SCIENCE

VOL. LXXI FRIDAY, JANUARY 24, 1930 No. 1830

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. MCKEEN CATTELL and published every Friday by

THE SCIENCE PRESS
New York City: Grand Central Terminal
Lancaster, Pa. Garrison, N. Y.
Annual Subscription, $6.00 Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

MEDICAL AND OTHER SCIENCES[1]

AN INQUIRY OF WHAT IS SCIENCE WHEN IS IT TAUGHT SCIENTIFICALLY

By Dr. A. J. GOLDFORB

PROFESSOR OF BIOLOGY, COLLEGE OF THE CITY OF NEW YORK

SCIENCE to-day plays an intricate, permeating and dominant rôle in our lives. It is not my intention to weary you with evidence in support of this thesis, to list the names of the various agencies engaged in science or the numbers of men and women so engaged or the ever-increasing millions of dollars expended, or to name the institutions, industries, occupations, mental attitudes and thoughts profoundly modified by science. Nor will I cite the figures of the ever-increasing numbers of "students" or numbers of hours or increasing budgets for science teaching from kindergarten to university. Nor is it necessary to list the amazing increase in the number of journals of science or the bewildering increase in the number of published manuscripts, the despair of the librarian as well as of the scientist. There is to-day probably no field of human endeavor which is not affected by the advances in science. Truly may it be said that science plays a dominant rôle in our lives.

It might then be assumed that the meaning of science, its essential characteristics, the tests by which it may be distinguished from pseudoscience or non-science, the methods of teaching science scientifically would be widely and clearly understood. The startling fact, however, is that science is probably more widely not understood or misunderstood than in any previous period of history. Misunderstood not only by the armies of schooled (so-called educated) masses, but by the teachers and practitioners of science. The extent to which unscientific science is taught in our schools is amazing.

[1] Address of the retiring vice-president and chairman of Section N—Medical Sciences, American Association for the Advancement of Science, Des Moines, Iowa, December, 1929.

It is widely believed that chemistry, agriculture, medicine, etc., are coequal with the sciences of advertising, linguistics, archeology, music, literature; that the science of paleontology belongs to the same brood as salesmanship and labor unionism; that the science of engineering is akin to the science of voice placement, taxonomy, crime, anatomy, history, religion, sociology, physiology, plumbing, Christian science, *ad nauseam*. One is amused, chagrined, disgusted, according to one's temperament.

All of you may admit biochemistry, physiology, mathematics into the realm of sciences. Some of you may grudgingly tolerate some branches of medicine, engineering, agriculture. Few will concede a place for history, philology, salesmanship. Probably none will yield a place to plumbing, Christian science, painting. But would there be agreement among you? Think of the disagreement among the schooled ("educated") members of our population.

Is science definable? Is it a fixed entity, like dollar, mile, gram; or is it a variable entity, whose limits may be defined, such as color, sound, taste, insurance rates; or is science an entity whose limits may not be clearly defined, such as instinct, thought, government, religion, art, etc.? Is it knowledge or only certain categories of knowledge or is it not knowledge at all?

Science, like paleontology and civilization, embraces a historical series from the simple to the complex. The earliest and most primitive stage may be characterized by the accumulation of new and exact facts or observations, but seen through eyes beclouded and distorted by beliefs in the ubiquitous and ever-present gods, demons, spirits dominating and controlling man and his environment.

A later stage in the development of science may be characterized by the systematization of such accumulated facts, or the formulation of "laws" such as the laws of alchemy, the Ptolemaic laws in astronomy, the pre-Galenic laws in medicine, the Aristotelian laws of nature. These were interesting mixtures of rare insight, exact and inexact description, mythology, hearsay evidence, unverified and unverifiable facts and laws—all subject to the whims of gods, saints and spirits.

The next stage in the evolution of science consisted in the gradual elimination of anthropomorphism and deism both from observation of facts and formulations of laws, but yet unverified and unverifiable, such as the bizarre pharmacopoeias up to the eighteenth century, the theories of evolution prior to Lamarck, the systems of classification including Linnaeus, the later alchemy.

Mathematics had in the meantime been developing most rapidly. Starting with immutable and absolute truths, the science of mathematics grew by further testing and elaboration of these fundamental truths.

The next great development in the natural sciences arose with the growth of chemistry and physics. It no longer sufficed to accumulate facts or observations, or to elaborate unverified and unverifiable laws or to hypothecate causes. The new era in science demanded experimentation, *i.e.*, the determination of the immediate or proximate mechanistic cause or causes of the phenomenon. The scientist started with observations or facts. He then devised an experiment in which the profoundly complex conditions associated with a phenomenon were reduced to constancy or were known. Only one condition or factor was unknown. The experimental conditions differed from the control conditions by this one, simple or compound, unknown factor. The object was to determine the rôle of this unknown or x in the phenomenon. He must produce or modify the phenomenon at will, to obtain the same results, under the same experimental conditions. From such experiments, new phenomena or new unknowns or new methods of analysis of the x arose, which new x must again be tested by suitable experimentation. The attitude of mind, the experimental procedure which enabled one to determine the unknown x, became the dominant characteristic of science. Facts were the initial step. The experiment as above defined became the sesame to solve certain riddles of the universe.

All these and intermediate stages in the development of science coexist, as do the different stages in the evolution of organisms or of civilizations. The confusion lies in the assumption that the aims, the methods, the mental attitudes, the values of each stage of science are the same. They are no more alike than are the ameba and man, the shepherd age of early Biblical times and the industrial age of 1900. Each stage is real, useful in its sphere, instructive, is a discipline, but they are not equal or alike.

The fact-seeking stage is the amebic stage in the development of science. The law-formulating stage is likewise primitive. The stage of unverifiable facts and laws also belongs to the primitive era of science. The experimental stage, provided it be properly defined, is not only more complex, more modern, but is different from the primitive stages of science. The experimental method, properly defined, characterizes real science and differentiates it from primitive science, from pseudoscience, from non-science, from anti-science.

A few examples may make the meaning more clear. If one describes or catalogues diseases into a nosological system or the species of animals or plants by the rules of classification or the parts of an organism by morphological, histological or embryological rules,

one is said to be engaged in science. By the same token the description and cataloguing into a rational system of pigments, lights and brushes would make one an artist. The description and cataloguing of dramas or poems should make one a dramatist or poet. If description and cataloguing of facts into a rational system constitutes science, then the dictionary, the encyclopedia, books of knowledge, outlines of science, survey courses, commercial descriptive catalogues of chemicals, of apparatus, of supplies, of dogs, of dresses, materia medica, hardware or any other body of systematized knowledge—then all these constitute the quintessence of science. Confusion is doubly confounded by including collections of facts in so-called science theses, projects and "researches," from grammar grade to university inclusive.

These collections of facts do not constitute science. At best they are the prelude to science, the building-blocks with which the structure of science is built.

It must not be forgotten that facts and laws are triumphs of a day, reigning to-day, dethroned to-morrow. To-day we believe in Euclidean geometry; to-morrow there is non-Euclidean geometry. To-day it is gravity; to-morrow relativity. To-day we worship calories; to-morrow vitamins and hormones. To-day we enthrone proteins; to-morrow amino acids. To-day we hail the telephone, to-morrow radio. To-day we have found absolute truth in the atom; to-morrow the ion and electron. To-day it is humors and *mal* air, to-morrow it is germs and mosquitoes. Facts and laws and truths change. That which reigns forever is the method of science, the experimental method, true to-day, to-morrow and all time.

It is alleged by many that expertness, or ability to do things resulting from knowledge, is science. Technical ability in making a good slide, or good dissection, a good surgical operation, a chemical analysis, an exercise in physics, constitutes science. By the same logic, the good bricklayer, plumber, builder, artificial flower maker, tailor, gardener, furrier, the butcher, the cook, the surveyor, the cement mixer, the analyzer of sputums and urines, etc., etc., all belong to the brotherhood of science.

It is not my intention to belittle the courses in so-called fact science or to minimize the usefulness of facts, or to detract one iota from the glory of the distinguished men teaching the sciences by this method. It is my intention, however, to point out as clearly as I can, that such courses, such methods represent the earliest, most primitive stages in the dawn of science. To substitute the accumulation of facts and "laws" or dexterity of manipulation for experimental methodology is naïve, erroneous, anti-science, the cartoon of science.

Do you realize how extensively this "fact" and "law" worship dominates our courses in science, not only in high schools, not only in colleges, not only in professional schools of dentistry, medicine, engineering, chemistry, but even in universities? Wherein is the factual method in these science courses different from the factual method in theology, esthetics, literature, art, history, etc.?

I have studied text-books, laboratory directions and laboratory manuals; I have talked with teachers of nearly all grades in all parts of the country, to find out to what extent real, scientific, experimental methodology is used in our laboratories. I find "scientific" dogmas, primitive fact-finding methods, automaton cook-book manipulations, the almost exclusive concentration on facts, more facts and yet more facts. The prevailing method is the archaic or Cambrian stage in the development of science. This archaic devotion to facts and cook-book manipulation parading under the banner of science pervades grammar school to university inclusive.

If time and occasion permitted I would like to cite the innumerable examples from laboratory manuals. In chemistry I can not distinguish any fundamental difference in method from that pursued by my cook in making a new recipe. In physics with elaborate tools for exact measurement I can not distinguish any fundamental difference in method from that pursued by the carpenter, mason, surveyor, builder, auto mechanic, also using tools of precision and following directions at least as intelligently. In biology and many medical courses, the student is told to observe this, that and the other structure, symptom or behavior. The same mental processes are involved as in pre-Darwinian, pre-Galenic days. The terminology and apparatus are different; the method of evaluating evidence is the same. The same emphasis on observation, the same acceptance of facts and theories, the same kind of assumptions concerning causes.

It is conceded that primitive science is a necessary stage in the training process. It is not conceded that primitive methods should occupy all or nearly all the years from grammar school to university.

In what courses of science is the student expected to frame his own question, find suitable materials (including bibliography) and apparatus, devise his own experiment, analyze the conditions, arrange one set of conditions where all are constant or known, another where only one condition or factor is unknown, to vary this unknown x, to solve for x, *i.e.*, to find the immediate cause of the phenomenon? To determine not facts or laws, but the condition or conditions under which a phenomenon can be made to appear? To get the same results, to deduce proper conclusions from the experimental data, to watch for

the crucial exception, so significant as a clue to further resolution of the constituent conditions, to plan the next experiment? Where is it taught that not facts or tools or materials or technique, but the method of experimental investigation is the test of scientific procedure?

It is not asserted that all science courses are conducted by the methods of the dark ages. There are beautiful examples of the highest type of experimental courses and investigation. It is asserted that such examples are ominously few. The astounding thing is that so many men, distinguished and honored for the splendid use of the best experimental methods in their own investigations, seem satisfied with the methods of bygone ages so far as the students in their courses are concerned.

One is astounded or amused by the wide use of the terms, the phraseology, the apparatus, the motions, the externalia of experimental procedure in our courses. These but thinly disguise the underlying dogma, cook-book manipulations, unverified or unverifiable conclusions, the methodology of non-science or anti-science. There is far too little of the real spirit of experimental investigation.

I take issue with the one who asserts that the methods of experimental investigation must be postponed to the Ph.D. thesis. I submit that the method can and should be used not only in the Ph.D. thesis, but also in most graduate science courses, in most undergraduate science courses, in most science courses of junior college, high school and grammar school. As a matter of fact, but far too infrequently, one finds splendid examples of true experimental investigation in all these grades of schools.

If it be contended that the spirit is willing but the flesh is weak, that real methods of experimentation can not be taught in classes of 100, 200, 400 and more, where part-time, inadequately and often narrowly trained student and graduate assistants do the teaching, I would agree wholeheartedly with you. If the large numbers of students, crowded schedules of the teachers, etc., prevent true teaching of science, then let it be proclaimed on every hand, on every occasion, let it be clearly understood by all, that what we are teaching is either not science at all or only the lower levels of science. Let it be clearly understood that we are training in modern laboratories, with modern tools and phraseology, by the standards of pre-Galen, pre-Vesalius, pre-Lavoisier, pre-Galileo, pre-Darwin, pre-Bacon, pre-Pasteur.

Maltraining like malnutrition, if long continued, has very serious and lasting effects on the organism—not only upon the large armies of college students but upon the relatively small number of selected (often self-selected) individuals who constitute the professional workers in science. My own experience as editor is in agreement with that of the editors of high exemplars of scientific journals. It is no secret, yet not widely known, how large is the proportion of manuscripts returned to authors not only for minor changes such as clearness of style, form, citations, English, etc., but for major and serious defects, such as inadequate controls, inadequate experimental procedure, inadequate proof that the phenomenon is due to the one variable cited, inadequate or overreaching conclusions from the data submitted. In other words, for lack of understanding of the basic qualities that constitute experimental science. It has been widely urged, and not facetiously, that even more rigid exclusion of manuscripts should be practiced.

Permit me to make brief reference to another related problem. For many years, probably from the time when Latin was the medium of communication by the learned, with increasing nationalism, the rapid specialization of the sciences, the multiplicity of societies, journals and meetings, the rise of science in more and more lands, with corresponding language difficulties, with the barriers created by hosts of new terminologies and elaborate technical procedures, there resulted a series of intellectual barriers that separated the workers into narrower and narrower fields. Such separatism is accentuated in our colleges and universities by the physical separation of departments and subdepartments in different parts of the campus, or, equally effectual, different floors of the same building, and much too frequently by unfriendly doors on the same floor. Each worker or group of workers conducts his researches and courses as though the allied divisions of science were wholly unrelated entities. And inevitably lack of understanding, misunderstanding, duplication, lost motion are correspondingly increased. To be sure, this separatism is not confined to the sciences.

In latter years there have been a number of movements directed to bringing together into cooperative action the workers in allied fields. This section, I believe, was the first among the sections in the American Association for the Advancement of Science to develop and champion such a movement. In 1920 I was honored by election to the secretaryship. During the next seven years, with the closest cooperation of the distinguished members of the section committee, we developed this program. We endeavored to bring together workers in related fields to discuss common and borderline problems. The section committee was selected so that one or more representatives of the different fields of medicine, parasitology, medical entomology, anthropology, vital statistics, veterinary science, medical practice, etc., were members of the committee. The programs were built on the same

principle, by careful inquiry of the section committee and others as to the important problems significant to workers in several fields, the outstanding investigators of these problems, the groups that should be invited to cooperate with us to the end that common or borderline problems might be discussed by workers in allied fields. The new policy met with immediate and wide and hearty response.

There were, of course, other efforts by other groups in the same general direction, a breaking down of ever-narrowing barriers, a regrouping and coming together of isolated groups. The Society for Experimental Biology and Medicine has for twenty-six years been bringing together the workers in the manifold fields of experimental medicine and biology. The geneticists are coming together more and more closely each year, and breaking down the artificial walls called zoology, botany, agriculture, etc. The National Academy, the American Philosophical Society, some of the state academies, are but a few illustrations. An interesting example is the union of chemists, physiologists, pathologists and biologists with the surgeons in the Mayo Foundation. The Rockefeller Institute more completely and on a larger scale than ever before (except in war) uses every field, every tool, every facility, and by frequent group meetings attacks the fundamental problems of health and disease. The Carnegie Institution is another illustrious example of coordinated attack on the problems of science. An increasing number of university laboratories, particularly in medical schools as well as industrial laboratories, are being manned and equipped with workers and tools from the allied fields of science. There is a wholesome trend in the same direction in respect to newly organized journals. More and more are the old barriers laid low, regrouping of workers, cooperative use of tools, techniques, cooperation of workers.

In the Cambrian or Precambrian age in which so many of our schools still live, one finds evidences of the ancient separatisms, the old fear of the trespasser, the vicious codes that separate related departments and subdivisions of science, that compel uncorrelated, compartmented, often antagonistic or contradictory facts, methods and results of science.

There are a number of movements making for concerted attacks on important problems by workers in allied fields. There are examples in the drama, in archeology, in exploration, in two or three colleges, in some university laboratories, in many research institutes, in a few grammar and high schools. These are oases surrounded and overwhelmed by the blare of publicity trumpets, proclaiming the polytheism of the sciences, shouting the shibboleths of experiment, project, research, scientific method, integration, survey courses and other fine names for rather poor substitutes of the original article.

When so many ills are believed to be curable not by medicine, but by legislation, we might urge a law like the Food and Drug Act, penalizing institutions of learning which put misleading labels on their wares —a law as unenforceable as many other laws.

May the time come soon when the practitioners of science, individually and collectively as faculties, may more widely and more adequately understand the aims, the methods, the importance of experimental investigation in science, its significance in education, in citizenship. We may then hope that trustees, presidents and heads of government laboratories will cooperate more and more in providing the conditions that will make for better, more thorough methods in teaching science, when emphasis will be transferred increasingly from the search for facts and "laws" to the search for rigorous experimental procedures.

We may then hope for a better understanding of science by larger proportions of our people, expect decreasing influence of faddists and stylists, less opposition and more cooperation from the public that conditions our lives, whether legislature, press, industrial and financial leaders, publicists, medical workers, etc.

We may then hope for a more rapid cure of the ills that the individual and democracy are heir to.

SOME OBSERVATIONS FROM LIMING INVESTIGATIONS[1]

By Professor C. A. MOOERS

TENNESSEE AGRICULTURAL EXPERIMENT STATION

Liming to increase crop production has been practiced in various parts of the world from time immemorial. The practice, however, appears to have been somewhat intermittent, and in Europe, as well as in this country, many farmers have never limed. The custom of liming gave rise to some disquieting adages, such as "Liming enriches the father but impoverishes the son" and "Lime and marl without manure make both farm and farmer poor."

Soon after the advent of the agricultural experiment stations liming became a subject of investiga-

[1] Address of the retiring vice-president and chairman of Section O—Agriculture, American Association for the Advancement of Science, Des Moines, December 28, 1929.

tion. The work of the Pennsylvania and Rhode Island stations is especially worthy of mention. At the latter station, studies were made of the effects of liming on the production of both field and garden crops, and of the relation of liming to different fertilizer materials, especially ammonium sulphate in comparison with sodium nitrate. Later the stations began to investigate the effects of liming on various soil components. Crop yields showed that judicious liming was generally beneficial, but it was essential to learn why this was so. It was also essential to discover harmful effects, if such existed. The solution of the problems involved required long-continued chemical investigations, such as have been pursued for nearly twenty-five years at the Tennessee station. Investigations were begun by the writer in 1905. About 1911 they were in large part—especially those relating to the soil reactions with the mineral elements—entrusted to Dr. W. H. MacIntire, the present head of the department of chemistry. Since that time he and his associates have untiringly, and successfully, pursued laboratory studies on the subject in connection with extensive open-air lysimeter equipments. During the progress of the studies it became necessary, as is often the case, to develop dependable quantitative chemical methods. Of the methods worked out in our laboratories, three have been adopted by the Association of Official Agricultural Chemists. The lysimeters have proved of great value in the determination of the outgo of the different elements over extended periods. The importance of time as a factor is too often overlooked. The forces that produce soil changes, like the mills of the gods, grind slowly. Where the subsoil is retentive, it is only by the use of the shallow lysimeter that the outgo of either nitrogen or the basic elements from the surface soil can be determined quantitatively. This apparently simple matter was not easily established. Precedent required tanks with several feet of subsoil. It was only after a detailed study of several years with lysimeter tanks of varying depths that the need and fundamental importance of the shallow tank was fully realized. This study was begun in the earlier years, and the outgo of nitrates following various manurial treatments furnished the conclusive evidence. In the liming investigations, therefore, two sets of lysimeters were used. One set contained only surface soil; the other contained the same amount of surface soil underlaid with a foot of heavy loam subsoil. The results from the two depths were often dissimilar, for even a foot of such a subsoil materially changes the outgo. The findings, though not always revolutionary, have furnished a substantial foundation on which to base explanations of some of the chemical reactions of the soil and have suggested new viewpoints of more than local interest. This paper deals almost entirely with results from the Tennessee station, but without citation to the more than fifty articles published by the station in bulletins and scientific journals.

Certain pioneer teachers maintained that liming with burnt lime had a "burning" effect on soil humus. The question has been a subject of field investigation since 1905, and a report from the first twenty years' study has been made. In addition to field plots, cylinders sunk in the ground and lysimeters have been utilized. In the field experiments, liming with burnt lime was found to accelerate materially the outgo of nitrogen and the oxidation of humus for a few years following the application. A retardation of these processes then set in, so that in the course of eight or ten years little difference was found between the humus content of the unlimed and the limed soil. In fact, the only noticeable final effect was where all crops had been removed without either the return of vegetable matter or the production of clover and grass. In such a case the limed areas were slightly lower in both nitrogen and humus.

The study with cylinders placed in the open showed that neither the oxide nor the hydroxide of calcium exercised any determinable chemical disintegration of the humus matter of the soil prior to carbonation or silication, even when heavy applications of eight tons per acre were made. In the case of an application of two tons per acre of burnt lime, maximum carbonation took place within five days and complete silication within ten days. Magnesium carbonate was converted to silicate even more rapidly than calcium carbonate.

One of the discoveries made in the earlier part of the work was the readiness with which silica, a supposedly inert substance, reacted with carbonate of lime—CO_2 being lost and calcium silicate produced. The general conclusion was drawn that the effects of an ordinary liming with burnt lime are derived primarily from silicates. It was concluded that, for soils formed *in situ* under humid conditions, magnesium was almost non-existent except in the silicate form. It was shown also that any toxicity induced by applications of the pulverulent magnesium carbonate was necessarily short-lived. In some of the earlier studies it was found that the silicates of calcium and magnesium produced greater clover crops than their corresponding carbonates—a finding that has since been verified at several other places. Silica additions were successfully used for tobacco as a "buffer" and to eliminate toxicity induced by heavy additions of magnesia.

Soil sulphur, along with soil nitrogen, is an element in plant nutrition that may undergo oxidation, and

may, therefore, be influenced by any material modification of the soil reaction or its supply of available calcium or magnesium. The effects of additions of these elements in each of several forms and at widely varying rates were thoroughly investigated over a fifteen-year period. Materially different effects were produced. The light applications of lime in all forms accelerated the outgo of sulphates. The acceleration was the greatest for the first year or two but continued in evidence throughout the period. On the other hand, the very heavy burnt lime applications retarded the outgo, not only through partial sterilization of the soil, but also by a fixation of the sulphate radical derived from both soil and rainwater. Magnesia increased the outgo at all rates of application. The increase was much the greatest at the heavy rates, and under this condition far surpassed that from lime in any form and at any rate. The experiments were carried out with two sets of lysimeters. One set contained about 7 inches of surface soil only, and the other the same amount of surface soil underlaid with a foot of a heavy loam subsoil. For a number of years the amount of sulphates leached from the surface soil alone was very much greater than that which came from the tanks containing the surface soil underlaid with the subsoil. The latter exerted a decidedly retardative effect on the outgo wherever burnt lime was applied. The same was true of the light applications of magnesia but not of the heavy applications, in which case the percolates soon became heavily impregnated with magnesium sulphate. Although the data show that liming increases the outgo of sulphates and that this increased outgo is still apparent after fifteen years, the final outcome can only be surmised. Reasoning from the effects of liming on the nitrogen outgo during twenty years of the cowpea-wheat experiments, there could be expected in the course of time a reduction of outgo due to the diminished supply of oxidizable sulphur. The data obtained are sufficient, however, to warrant the consideration of a possible sulphur shortage for plant nutrient purposes wherever liming is long practiced without sulphur additions from fertilizer or other sources. As a matter of interest and warning it may be stated that the percolation results would have been inconclusive, if not misleading, had not determination been made of the sulphate radical as well as calcium, magnesium and potassium brought by the rainfall.

The findings relative to sulphur as found naturally in the soil were amplified by a similar study for twelve years following additions of ferrous sulphate, pyrite and flour of sulphur. Where ferrous sulphate was applied to the soil, both burnt lime and magnesia increased the outgo of sulphates. Where pyrite was applied, the liming materials tended to depress the outgo. Where elemental sulphur was applied, both burnt lime and magnesia in the lighter applications and magnesia in the heavy applications accelerated the outgo. It is probable that the increased sulphur outgo produced by the liming of a humid soil comes as a result of increased oxidation of organic sulphur.

The literature of the past makes frequent reference to the supposed liberation of soil potash by liming. This appears to have been largely a surmise based on the mass-action effect that was observed when acid soils were suspended in solutions of neutral calcium salts. Plot results were available for each of several widely different types of soil on which liming experiments had been continued from seven to twenty years. The crop yields on the twenty-year field had long shown a greater potash deficiency for the limed areas. It was a question, however, whether this was entirely due to the heavier drafts on the soil potash as a result of the larger crops produced in the earlier years by the liming or in part to reduced availability of the potash. The answer to this question was furnished by the lysimeter studies. It should be stressed that in these studies no crops were grown, hence the quantity of potash in the percolates showed the influence of the various additions of calcic and magnesic materials upon the availability of the soil potash. Ten years ago the Tennessee station published the results from five years of these lysimeter experiments, which showed that in none of the soils used, with a single exception, did liming liberate potash. The exception was where enormous amounts of calcium hydroxide persisted for over two years as the result of 100-ton calcium oxide additions, and even in this case there was only a small and temporary liberation of potash. Ordinary applications of both lime and magnesia repressed the potash content of the soil leachings. The repressive effect of liming upon the solubility of native supplies of potash was likewise observed where potash was added through incorporations of red clover.

The Tennessee station's findings relative to the repressive action of liming on the availability of soil potash have recently been verified by reports from several other stations which showed a lower potash content for plants grown on limed soils. The evidence, as a whole, may be said to show that liming forces potash back into the soil, and not out of the soil as some German investigators have asserted.

The outgo of calcium and magnesium as influenced by different liming materials has been considered in lysimeter experiments for a period of fifteen years. According to the law of mass action it might be expected that calcium would displace magnesium, and *vice versa,* and it was long assumed that this is what

took place. The experimental results can be very briefly stated, and are quite at variance with the displacement theory. Liming with burnt lime and high calcic limestone appreciably increased the calcium outgo and depressed the magnesium outgo. In a similar manner applications of magnesium, either as oxide or as carbonate, increased the magnesium outgo and very markedly depressed the calcium outgo. The result from liming with dolomite—a double carbonate of calcium and magnesium—is of special interest. It was found that in spite of calcium enrichment supplied by this material there followed a decreased outgo of calcium and an increased outgo of magnesium. It should be borne in mind that the sum of the enhanced outgo of calcium and magnesium was found to be nearly constant. These findings have an especial interest in explaining the detrimental effect that high-calcic limes may exert upon such a magnesia-loving crop as tobacco. They throw a new light upon many of the interpretations given to the older studies of lime-magnesia ratios.

Study of the accumulated data for calcium and magnesium outgo from economic liming in the lysimeter experiments shows that the loss of lime is too small to account for the decreasing benefit to legumes of high lime requirement. A need for reliming is therefore apparently due to changes in the form or state of the large residual fraction of added lime rather than to extensive outgo in those soils that possess good fixing capacity. It has been demonstrated to our satisfaction that after added lime is absorbed by the soil the absorption product undergoes a progressive decrease in availability. This phenomenon may be a simple "aging," using the term in the chemical sense, for which there are many analogies in the formation of precipitates that in time become increasingly less soluble.

Much has been said relative to base-interchange—a phenomenon that is of great importance in the genesis and subsequent make-up of dyke-reclaimed soils and those of arid regions. Under humid conditions Tennessee soils do not show base-interchange reactions in the zone of incorporation of calcic and magnesic materials, even in the presence of high concentrations of a neutral calcium or magnesium salt; but on the contrary they show a repressive action. On the other hand, the percolates from lime- or magnesia-treated surface soil do exert a base-interchange effect in the subsoil where the action of sulphates, nitrates and chlorides comes into play beyond the zone of contact between the soil and additions.

The writer has reserved for the close of this paper a brief discussion of a most interesting and important soil reaction which explains certain soil phenomena and also has other and unexpected applications as an important analytical procedure both in the soils laboratory and in industry. That is the formation of the ternary systems, $CaO-Fe_2O_3-CaSO_4$ and $CaO-Al_2O_3-CaSO_4$. These compounds and the reactions involved were new to soil chemistry, and their discovery is one of the more interesting achievements of the station's researches.

In connection with certain lysimeter experiments it was observed that very heavy liming—100 tons of CaO per acre—practically inhibited the outgo of sulphates for a year or two, but that this effect was not permanent. The phenomenon was studied from various angles, and finally by means of a synthetic study with pure gels, the formation of compounds of both iron and aluminum united with CaO and $CaSO_4$ was found to be the explanation. These compounds are of low solubility as long as the system is alkaline, but are readily soluble in either neutral or acid media. Further investigation showed that aqueous solution of calcium hydroxide and calcium sulphate readily attacked aluminum complexes in the soil, with the formation of the ternary compound. Afterward the reaction was utilized as the basis of a chemical method of determining the amount of reactive alumina and silica and promises much as a useful yardstick in the measurement of the colloidal properties of a soil. Industrially the discovery is valuable because an explanation is given for the disintegration of concrete under certain conditions, which had previously not been understood, and furnishes the necessary information for the avoidance of such trouble in the future.

In conclusion, the fact may well be emphasized that the soil solution is the source of all the nutrients taken up by the roots of plants. As Brazeale has said, "The plant deals with the soil solution and with the soil solution only." It is this solution that is obtained by means of lysimeters. Lysimeter findings may therefore be studied to advantage by plant physiologists and others who are interested in learning what effects manurial treatments may be expected to exert upon crop-nutrient assimilation and plant-ash content.

A DEVELOPING VIEW-POINT IN OCEANOGRAPHY

By Dr. HENRY B. BIGELOW
MUSEUM OF COMPARATIVE ZOOLOGY, CAMBRIDGE, MASSACHUSETTS

AT the Princeton meeting of the National Academy of Sciences on November 18 last, its committee on oceanography, consisting of W. Bowie, E. G. Conklin, B. M. Duggar, J. C. Merriam, T. W. Vaughan and F. R. Lillie (*chairman*), with H. B. Bigelow, *secretary*, submitted a report on its study of the scope, economic

importance and present status of oceanography, with recommendations as to how this science may more effectively be encouraged in America. To the general scientific public the most significant feature of the report is perhaps the general conclusion, reached by the committee, that the establishment on our Atlantic coast of a new organization dedicated to the encouragement and prosecution of oceanographic investigation is "the greatest need at the present time, both from the point of view of American oceanography and also for adequate participation of this country in a study necessarily international." The committee concludes further that "the establishment and endowment of an Atlantic Oceanographic Institute should be realized at the earliest possible moment."

The report is too long to be summarized here.[1] We, therefore, think it pertinent to set forth something of the view-point developed in the report that led to the recommendation of this particular kind of support for oceanography, rather than to the more conventional suggestion that our knowledge of the sea would be most rapidly increased by more deep-sea expeditions, and greater.

This commitment implies, as is indeed the fact, that oceanography has of late entered a new intellectual phase, to explain which a word of retrospect is necessary. While in one sense this is among the oldest of natural sciences, in another it is one of the youngest. To date its absolute birth is an impossibility, for this took place when first some fact about the sea was not only observed but recorded: certainly this happened many centuries before the Christian era. And with the passage of the centuries recorded knowledge continued to accumulate about one phase or another of what we now call oceanography. But this could hardly have been dignified with the rank of a separate science down to the early years of the century just passed, because it had not yet passed the stage of collecting and recording isolated facts, whether about the surface of the sea, about its inhabitants in shallow waters or about its margins. In fact all that lay below the surface of the open sea, or more than a few fathoms down from tide-line around its shores, continued, down to the late seventeen hundreds, as much a mystery as it had been to the Phenicians, except when some deep-sea fish was cast up upon the strand. Indeed, it was not until about the middle of the nineteenth century that systematic examination even of the surface of the sea was seriously undertaken, or that scientists awoke to the fact that the underlying waters offered a whole new world for exploration, that offered no unsurmountable difficulty.

Evidently this descent into the abyss could not be made without the development of suitable instruments, whether to plumb the depth, to sample the living creatures there or to measure the physical and chemical characteristics of the water. And philosophically it is interesting that it was the birth of this new view-point, reached from gleanings with very primitive gear during the preceding fifty years or so, which led to the development of efficient instruments—not the reverse.

It would, indeed, have been quite within the technical abilities of the Romans of Pliny's day to develop the depths of the Mediterranean and to explore its biota, though of course examination of the temperature and salinities of the sea must in any case have awaited the development of the sciences of physics and chemistry as we now know them.

Students of the history of science may well date the birth of modern oceanography from December 21, 1872, the day when the *Challenger* set sail from Portsmouth, England, on her memorable voyage. And thenceforth, with every fresh venture below the surface of the sea, such a flood of new facts came pouring in that it seemed for a time as though this fact-catching could never lose its novelty. One great deep-sea expedition led to another, and more was learned about the sea during the last thirty years of the nineteenth century than had been during the preceding three thousand. But after a time, as so often happens when some scientific discipline takes a sudden spurt, this fact-catching began to lose something of its freshness.

At first, when no one knew what lived on the bottom of the sea, every new fish that was drawn up was a marvel. But now we have come to have a more sophisticated outlook upon such things. Students began, in short, to feel that the mere accumulation of facts from the sea, when there is an inexhaustible supply, may actually become a bit sterile, just as catching fish is to a sportsman where fish are too plentiful. To maintain interest under such circumstances, one must need the fact—or the fish, as the case may be. So it was natural that when persistence in the old methods no longer yielded startling discoveries, signs could be seen of the approach of a period of stagnation, following the peak of fevered activity. And oceanography would probably be in a moribund state in America to-day, just as the art of sailing a square-rigger is, but for the birth of the new idea that what is really interesting in sea science is the fitting of these facts together, and that enough facts had accumulated to make the time ripe for an attempt to lift the veil that had obscured (and still obscures) any real understanding of the marvelously complex and equally marvelously regulated cycle of events that takes place within the sea.

The foundation for this conscious alteration in

[1] It is now available in mimeographic form.

view-point, from the descriptive to the explanatory, was a growing realization (this could have come only after multitudes of facts had been accumulated) that in the further development of sea science the keynote must be physical, chemical and biological unity, not diversity, for everything that takes place in the sea within the realm of any one of these artificially divorced sciences impinges upon all the rest of them. In a word, until new vistas develop, we believe that our ventures in oceanography will be most profitable if we regard the sea as dynamic, not as something static, and if we focus our attention on the cycle of life and energy as a whole in the sea, instead of confining our individual outlook to one or another restricted phase, whether it be biologic, physical, chemical or geologic. This applies to every oceanographer: every one of us, if he is to draw the veil backward at all, must think and work in several disciplines. He must be either something of a Jack of all trades or so closely in tune with colleagues working in other disciplines that all can pull together.

We see here a case where economic pressure was largely responsible for lifting a field of knowledge, willy-nilly, to a higher and more rarefied atmosphere of what we flatter ourselves by calling "pure science"—in this instance the plight of the commercial sea-fisheries of northern Europe. The countries bordering on the North Sea had exploited the resources of the fisheries for centuries. And it chanced that just when oceanography was enjoying its nineteenth century boom, the fisheries were also so rapidly expanding in intensity, through the development of more effective fishing methods, that the dread of overfishing became imminent. What more natural than for the maritime nations to turn for advice to the sea scientists who had for the past fifty years been so busily carrying on explorations of the sea, naming and classifying the fishes—especially when the deep-sea expeditions had been so largely supported by taxpayers' money?

But sound advice as to the wise management of the fisheries science could not give, for while volumes of facts had been garnered about the individual species of food-fishes, and as many more about the medium in which these live, there had as yet been no general and concerted attempt to fit these two categories of facts together, or in other words, to unravel the skein of factors that controls the lives of fishes in the sea. It was, however, clear enough, once attention was directed thereto, that if the exploitation of the deep-sea fisheries was to be based on sound, rather than on hit-or-miss, principles of conservation, it demanded just this understanding of the lives of its victims; it was equally clear that such an understanding could be gained only through a synthesis of studies in many fields, and that no mere piling up of data, however extensive, could yield it.

All this was so obvious, once pointed out, and the economic urge was so pressing that a vast amount of attention (and a vast amount of money) has been expended on fisheries-biology during the past quarter century, and an appreciation of the need of unity has come to permeate the whole edifice of oceanography. But even with this new view-point and new impetus, science has given assistance much more slowly to the suppliant fisheries than had been hoped, because the requisite synthesis of knowledge has proved a task immensely difficult intellectually as well as technically, and because we are only now beginning to appreciate the appalling complexity of every marine problem—a complexity which if discouraging in one way offers in another way a most alluring stimulus. Speaking as a biologist, the proverbial "way of a maid with a man" is glass-clear as compared with the way of a fish in the sea, for in the sea there is no such thing as a hermit fish—or fact: every sea animal depends on an endless chain of other animals or plants and equally on an endless chain of facts and events in its inanimate surroundings.

Though we have been studying cod, mackerel, lobsters and the rest, with men and ships at sea, in libraries and laboratories ashore, with hopes and with discouragements, for all these years, we do not yet know all the links in the life chain of any single species, or even which links are apt to be the most important, or even where to seek more than a few of these links. But oceanographers do now very thoroughly appreciate that the geophysical and chemical links are quite as vital—perhaps even more so—than those of the sort more usually named biologic.

When one picks up a fish, one may be said, allegorically, to hold one of the knots in an endless web of netting of which the countless other knots represent other facts, whether of marine chemistry, physics or geology, or other animals or plants. And just as one can not make a fish-net until one has tied all the knots in their proper positions, so one can not hope to comprehend this web until one can see its internodes in their true relationship. This is to-day the conscious aim of oceanographers.

To look more closely at some of the lines of knots in the web will perhaps help to bring this conception home.

On the whole, the simplest of these to follow, and certainly among the most important, are the food lines, *via* which one species is interdependent upon another. Thus a shark may eat a pollock, a pollock may eat a smelt, a smelt eat an amphipod, an amphipod eat a copepod and a copepod eat a diatom, and each must

find its food-species at hand or perish. Or followed in the reverse description, we see here, as clearly, an enemy-prey line, for amphipods, if plentiful enough, may locally exterminate copepods, or fish locally decimate amphipods. And big fish may so prey upon little ones (even of their own species) that this last factor must seriously be taken into consideration in studying the maintenance of stocks of fishes on certain of our most productive fishing banks, for there is no prejudice against cannibalism in the sea; that is a purely terrestrial invention.

Nor can the biologist focus narrowly upon the biologic lines in this allegoric web, the chemist on the chemical, for when any animal is hatched into the sea and throughout its life, its survival depends quite as much upon whether its physical and chemical surroundings are favorable as upon the immediate food supply, or presence of enemies; while in life and in death marine organisms profoundly affect their aqueous environment. This interdependence, by and large, is much more intimate in sea than on land, for reasons that there is not space to enter upon here. Marine animals and plants being, as a class, much less effectively protected against their environment (because the marine aquatic environment is a favorable one, not an unfavorable, as is the air), the balance in this respect is often a very delicate one indeed. For examples of this, one need only turn to the tremendous fluctuations that are known to take place from year to year and over periods of years in the numerical abundance of various fishes in the sea, from purely natural causes with which the hand of man has had nothing whatever to do. It will be enough here to refer to the proverbial ups and downs shown by the mackerel. In the eighties of the past century American waters swarmed with mackerel: the sea was full of them. They then dwindled in abundance (though with ups and downs) until, at their low point, in about 1910, the mackerel might almost be called a scarce fish. But then the stock began to build up again, in spite of the fishery, until to-day it seems that we may look forward to another period of tremendous plenty in the near future. Annual fluctuations, almost equally spectacular, are well known to have taken place among the herring and among many other species.

We now realize that these periods of plenty and of scarcity in essence reflect good and poor years of production; that for many fishes a year of good production is decidedly an unusual event, so that a given year-class may dominate the stock over a considerable term of years, whereas other species may maintain a more nearly even population. And we begin to be able to predict, from the number of young fish produced and surviving in a given year, the probable productivity of the fishery in years to come. But we are still totally in the dark as to the exact causes of such fluctuations, except that they certainly are not inherent in the vital nature of the species concerned but have to do with external circumstances, or events, in the end physical-chemical.

The most obvious line of connection between the biological and the physical-chemical realms in the sea is *via* temperature; no creature can live, much less thrive, if the water be too hot or too cold. But even as seemingly simple a constant as temperature can not be considered *per se,* or as an adjunct, in the sea, because water has no inherent temperature, but is given the latter by a complex of such factors as solar radiation, back-radiation to the air, evaporation and the melting of ice. Consequently, in our examination of temperature, we are led without a break into the fields of astrophysics, of meteorology and of polar geography. We are also led, very abruptly, to a consideration of the circulation of the sea, because the temperature there at any given time and locality is largely controlled by the currents, as the latter transfer cool or warm water-masses from place to place. There is, too, a direct mechanical connection between ocean circulation and the lives of the marine inhabitants quite as important as that *via* temperature, for currents also carry plants and animals about, likewise other materials of all sorts. Currents, in fact, play much the same rôle in marine economy as do railroads, or any other transportation system on land.

We must realize that, wonderful medium though sea water be for the support of life, any animal or plant would soon exhaust the vital possibilities of the water in its immediate vicinity unless some transportation system were in operation, either to carry the creature elsewhere (whether voluntarily by its own activity, or involuntarily) or to bring to it new water holding in solution or in suspension the substances that the organism in question needs. For the latter sort of transport, the currents and drifts of the sea are wholly responsible; largely so also for the former, by effecting the involuntary migrations of creatures young and old, a kind of dispersal that is constantly going on, and on a scale much broader than is generally appreciated. If the life of the eel is perhaps the most spectacular instance of this type of migration that has yet been followed through to its conclusion, thousands of other kinds of sea animals and plants equally owe their geographic distribution (presence here and absence there), and their dispersal from the regions where they were produced to other regions where they pass the greater part of their lives, directly and solely to mechanical transport by ocean currents. This category of travelers includes the

majority of our important food fishes, for most of these, when young, drift at the mercy of tide and current for considerable periods. We think here not only of thousand-mile migrations such as those of the young eel but also of shorter travels such as those of the codfish, by which the little fry are dispersed from the inshore grounds of their nativity to offshore banks, where they grow and fatten for the nets or hooks of the fishermen, and of similar events in the lives of the haddock, of the herring, of the mackerel, of the plaice and of a host of others.

In our allegorical web of the sea, the current-lines also lead in many other directions. Currents of a sort not so familiar (*i.e.*, vertical) are solely responsible, for example, for the aeration of the deeps, without which all but the uppermost stratum would be a waste more desert than the Sahara. Currents, too, largely control the distribution of salinity over the oceans; they wear down some coast lines and build up others; they distribute sediments over the bottom of the sea; they affect indirectly and directly the comings and goings of every seaman who sails the sea, and they so largely determine the climates of the continents and the system of winds that there is no possible way to disentangle oceanography from climatology.

Reasons as cogent as these make even the biologist admit, no matter how strictly he may confine himself to his own narrow niche, that the currents of the sea offer to-day one of the most intriguing fields of study in sea science. And this is true not only from the descriptive side (for we still have much to learn even about the characteristics of the larger and more impressive ocean currents—Gulf Stream, for instance—let alone the obscure) but from the standpoint of the physical forces that keep the circulation of the sea in its closed and continuous operation. So the unfortunate biologist, even if mathematics are to him a closed book, as is the case with too many of us, must perforce take as keen an interest as do his physical confrères, and hold as high an appreciation of the modern applications of mathematics to oceanic dynamics.

The interrelations between the chemical and the biological phases of oceanography are quite as close as those just mentioned, again, perhaps most easily illustrated *via* the food-line. Even the tiniest animal in the sea must have organic stuff on which to feed. And we have been in the habit of thinking that sea-animals, like land, must depend in the long run on green plants for their pastures. Perhaps this is not an invariable rule, for it is possible that the autotrophic bacteria may prove to short-circuit this line more generally than now seems likely. But in the main, animal life in the sea does depend for its existence on the ordinary marine plants, chiefly on the planktonic plants. The problem of the food supply of these plants falls directly within the province of the chemist, for the only sources for plant food in the sea are the substances dissolved in the surrounding water. Consequently the biologist is as intimately concerned with the sea water itself as is the geochemist. Whether for the one or for the other, most of the basic problems of oceanography focus around the fact that the oceans are filled not merely with water, but with water that is "salt."

Though sea water may be named the most complex of all common substances if judged by the number of its constituents (for doubtless it contains, in solution, all the known elements), it is on the other hand the most uniform of all common substances, next to air, as measured by chemical analyses of ordinary delicacy: Sea water is sea water from whatever ocean we may draw it. And one of our most interesting oceanic problems of to-day is how this uniformity is maintained against the great variety of processes that are constantly working to destroy it. Here, again, the disciplines of the biologist, of the geochemist and of the geophysicist unavoidably unite, for some of these processes belong in one, some in another of these scientific subdivisions.

This is self-evident; for example, the diluting effect of the rain that falls upon the surface of the sea or of the fresh water that is poured in by rivers, and the concentrating effect of evaporation, all offer problems in physics. Consider, again, the chemical problems that center around the fact that the solutes contributed with river water are very different in their compositions from sea salts; around the withdrawals of lime, of silica, even of strontium by animals and plants in the formation of their shells; equally around the withdrawals of food stuffs by plants, balanced against the contributions to the water of other stuffs as carcasses decay; or around the alterations in ionic dissociation that result from such additions and withdrawals. Only in conjunction can chemist, geologist or biologist hope to learn how the sea remains so constant through it all that we must analyze to parts per million, even to parts per thousand million, before we can express the existing variations in the relative proportions of its different salts; or how it is that the alkalinity of the sea never varies outside the narrow range in which protoplasm can live—is in fact as delicately balanced as the alkalinity of our own blood serum. It is only by a synthesis far more thorough than has ever yet been accomplished that we can ever hope to understand the working of the marvelous and self-operating regulatory system that maintains this balance now and that we may be sure has maintained it through past geologic ages.

From whatever point in our allegorical web we may choose to commence, whether geophysical, chemical or biologic, if we proceed far enough in our exploration we are inevitably led into the province of the geologist (and *vice versa*), for the oceanographer, if he is to be anything of an architect (not a mere bricklayer) must equally concern himself with the shapes and with the structure of the basins that hold the oceans.

As truly as the character of the bottom of the sea largely determines what kinds of animals live thereon (which every fisherman knows to be the case), biological processes going on within the water as largely determine what sorts of sediments shall accumulate in any given place to make up this bottom, except around the coasts, where these processes are masked by silt washed down from the land. Consequently the problems of sedimentation in the ocean fall as much within the province of the biologist as of the geologist. Thus the oozes that accumulate over the greater part of the sea floor consist of the skeletons of animals and plants that sift down after death from the upper layers.

Where, when and in what quantity these skeletons start to sift down is a problem as strictly biologic as any, for it depends in part on the geographic distribution, equally on the birth and death rate of the particular plants and animals concerned. But whether and in what quantities these skeletons do actually reach the bottom is a physical-chemical question, as is the ultimate fate of such of them as do arrive there. So, too, is their effect upon the bottom water of the ocean when they go back into solution, for given time enough anything will dissolve in normal sea water. But at the same time, the alterations caused in the sediments by the entrapped water offer very important and far-reaching geological problems, for while we know that the limestone and shale rocks were originally laid down under the water under conditions comparable to those of to-day, they differ greatly in their present state from the muds and oozes that are now accumulating under the seas. Two of the most pressing problems in this field concern the method of formation of petroleum and of iron ores. In fact, no one will dispute that the study of the modern sea bottom is a geologic necessity, for only by this means can geologists hope to understand how the different classes of sediments now solidified into rock were originally accumulated and subsequently metamorphosed.

The shapes of the oceans, too, confront the oceanographer at every step, whatever be his particular chosen field of research, because it is the contours of the coast lines and of the submarine slopes that confine and control the whole system of submarine circulation, however it may be kept in motion. And as every oceanographer realizes, this circulation is in the end the life blood of all the events that take place in the sea.

There is, I think, no need to quote more examples to show that the different disciplines of oceanography inevitably interlock, or to prove the intellectual necessity of not only recognizing but indeed acting upon this unity, if we hope ever to gain a thorough understanding of the sea and its inhabitants. Any attempt (conscious or unconscious) to hold them apart can result only in frustrating this high aim and in setting us backward to the stage of simply gathering and accumulating facts in unrelated categories.

OBITUARY

WILLIAM A. ORTON

DR. WILLIAM A. ORTON, scientific director and general manager of the Tropical Plant Research Foundation for the past five years, and formerly senior pathologist in charge of the office of cotton, truck and forage crop disease investigations at the Bureau of Plant Industry, U. S. Department of Agriculture, died at his home in Takoma Park, D. C., on January 7. He was in his fifty-third year, having served slightly more than twenty-five years in the department at the time he resigned to take up the tropical research work. Funeral services were conducted at the Takoma Park Presbyterian Church on January 9, and interment was in Rock Creek Cemetery, D. C.

The death of Dr. Orton closed a brilliant career, one marked during its later years by a courageous struggle against ill health which was a marvel to his associates. In spite of these limitations, he was a leader in the field of plant pathology and tropical agriculture, and has accomplished results of outstanding importance in the thirty years since he entered the Department of Agriculture.

Dr. Orton was born on February 28, 1877, at North Fairfax, Vermont, and was graduated from the University of Vermont with the B.S. degree in 1897. After a year of graduate work in the University of Vermont, specializing in botany and plant pathology, he received his M.S. degree in 1898. In 1915 the degree of D.Sc. was conferred upon him by the University of Vermont. He entered the Department of Agriculture on June 1, 1899, two years before the establishment of the Bureau of Plant Industry, and

was prominently associated in a constructive way with the activities of the department, particularly in lines related to plant pathology, for a little more than a quarter of a century.

During the first decade of his service in the U. S. Department of Agriculture he carried on researches on diseases which were causing serious losses to cotton, cowpeas and watermelons in the southern states, and he was a pioneer in the work of breeding and selection which resulted in the development of disease-resistant varieties that have been such great factors in reducing wilt and root-knot losses in these crops.

In 1907, soon after the organization of the Bureau of Plant Industry, he was made head of the office of cotton, truck and forage crop disease investigations, and held that position continuously until he resigned from the department in November, 1924. During this period the work under his charge gradually grew until it included research projects on the diseases of most of the important vegetable and forage crops. Potato diseases especially received major attention and largely as a result of work done under his direction the infectious nature of the numerous virus diseases of potatoes was discovered and methods of control through the use of certified seed were worked out. Dr. Orton was instrumental in initiating and promoting the now accepted methods of potato seed certification which have been of enormous value to the potato industry of this country.

Dr. Orton organized and for several years carried on the plant disease survey of the Bureau of Plant Industry, which has become of great importance in connection with work on the control of crop diseases. During the World War he was active in organizing research work on diseases causing losses of vegetables in transit, market and storage, which was developed in close cooperation with the food products inspection service of the Bureau of Agricultural Economics and very materially increased knowledge of the causes of losses in shipment and marketing. As a result of this work methods for the reduction of disease losses in the process of marketing have been developed.

He was also active in the work leading up to the passing of the plant quarantine act, and for the last twelve years of his departmental service he was vice-chairman of the Federal Horticultural Board.

In addition to his plant pathology investigations, Dr. Orton privately carried on studies of food plants with a view to providing a more varied and suitable diet for invalids, especially diabetics and neurotics, whose range of foods is restricted.

He was very active in the organization of the American Phytopathological Society. He was its president in 1921, and for several years editor-in-chief of its official journal, *Phytopathology*. Also he was a charter member of the American Horticultural Society organized in 1922 and a member of the following scientific societies and clubs: The American Association for the Advancement of Science, the Botanical Society of America, the Botanical Society of Washington, the Washington Academy of Science, the Society of Horticultural Science, the American Society of Agronomy, the American Genetics Association, the American Society of Naturalists, the International Society of Soil Science, the Société de Pathologie Végétal, the Society of Foresters, International Society of Sugar Cane Technologists, the World Agricultural Society, the Cosmos Club and Phi Beta Kappa.

In November, 1924, he resigned from the department to become scientific director and general manager of the Tropical Plant Research Foundation, an organization initiated by a committee of the National Research Council, of which he was chairman, for the purpose of promoting the study of the plants and crops of the tropics and their disease and insect enemies. The principal projects under way are those on sugar-cane production problems in Cuba with the support of the Cuba Sugar Club; a survey of sugar-cane and cotton industries of the west coast of Peru; an investigation of chicle production problems of British Honduras, a forestry survey of portions of Cuba and a survey of the timbers of tropical America. He was also technical adviser to the division of agricultural cooperation of the Pan American Union. In connection with the establishment and carrying on of this work of the foundation Dr. Orton made numerous trips to Cuba and one trip to Brazil in the interest of the development of a national forest service in the Brazilian Department of Agriculture.

The Inter-American Conference on Agriculture, Forestry and Animal Industry, which is to be held in Washington in September, 1930, under the auspices of the Pan American Union, grew out of recommendations which were presented by Dr. Orton through the Pan American Union to the sixth International Conference of American States, which met at Havana in 1928. He has worked actively in the preparation of the program which will be followed at this conference.

Dr. Orton was a man of unusually broad vision and clear judgment as to the needs of agricultural science in the United States and tropical America. Coupled with this was outstanding, original ability in the formulation of sound workable plans for the accomplishment of desired results. He recognized the value of cooperation for the attainment of scientific results, and had the personality to make and keep friends, so essential in such an undertaking. His influence on young men entering the field of scientific endeavor

has been outstanding, and has left with them lasting inspiration. He was always kindly and considerate of others, of even temperament and easy to work with and his optimism was unbounded. His acquaintance was broad and his friends are many.

Dr. Orton's publications, largely on subjects related to plant pathology, comprise some forty or more bulletins and circulars of the Department of Agriculture and many published in outside journals and magazines and by the Tropical Plant Research Foundation.

Dr. Orton is survived by his widow and two daughters, Alberta and Alice, and by two married sisters, who reside in Vermont.

W. W. GILBERT

SCIENTIFIC EVENTS

SINANTHROPUS PEKINENSIS

THE Peking correspondent of the London *Times* reports that at an open meeting of the Geological Society of China held on December 28 the closely guarded details of the finding in North China of the skull of a man hundreds of thousands of years old were officially revealed. The discovery, which is claimed to be the most important of its kind, was made on December 2, in a limestone cave deposit at Choukoutien, forty miles from Peking.

The find is said to be a unique specimen, and consists of the greater part of an uncrushed adult skull belonging to an entirely new genus, known to science as *Sinanthropus Pekinensis,* which is definitely placed above the Java ape-man in brain capacity, but below Neanderthal man. The Peking man is considered to antedate Neanderthal man and is held to be nearer the genus *Homo* than the Piltdown and Java types. Estimates of the age of the skull vary greatly. Dr. Grabau, adviser to the Chinese Geological Survey, states that the Peking man lived at the beginning of the Quaternary Period and gives his age as 1,000,000 years, but Père Teilhard Dechardin, president of the Geological Society of France, and also adviser to the Chinese Survey, favors an estimate of 400,000 to 500,000 years.

The credit for the actual discovery of the skull goes to a young Chinese geologist, Mr. W. C. Pei, in charge of the field work of the Geological Survey at Choukoutien last season. Excavations there had previously yielded the major parts of the two lower jaws and numerous teeth and skull fragments of "Peking Man," as well as four tons of fossil remains, including the sabre-toothed tiger, which flourished at the same time as "Peking Man." The skull is still largely embedded in hard travertine, which will require a couple of months of difficult and delicate work to remove, but the vault from the massive brow ridges to the occiput and the whole of the right side have already been freed from the relatively soft matrix, showing that while most of the facial region seems lacking, the brain case is almost completely preserved. The massive jaw sockets are also visible.

Compared with the Java skull which is approximately the same length, the Peking skull is said to possess characteristics which point to relatively greater brain capacity.

CANADIAN NATIONAL RESEARCH LABORATORIES[1]

TENDERS have been invited by the Government of Canada for the construction of a National Research Laboratories building that will cost, when finished, approximately three million dollars. Appointments of chiefs to two of the laboratory divisions has been announced.

Dr. H. M. Tory, formerly president of the University of Alberta, and now the president of the National Research Council, has expressed the view publicly that the new home for research in Canada will be one of the finest to be found in any country. It is being built on the banks of the Ottawa River in the capital city. Designed in the form of a giant figure "8," it will stand 60 feet (four stories) high, 418 feet long, and 176 feet deep. Two hundred and fifty thousand feet of floor space will be provided. Library accommodation will be for 300,000 volumes. An assembly hall and associated rooms will be capable of accommodating the staff and the various scientific societies of the Dominion.

Plans call for the development of the following divisions: The divisions of physics and engineering physics, to the head of which Dr. Robert William Boyle, dean of the faculty of applied science at the University of Alberta, has already been appointed; the division of industrial chemistry, to the head of which Dr. George Stafford Whitby, professor of organic chemistry at McGill University, has been appointed; the division of economic biology and agriculture, to which Dr. Robert Newton, professor of field crops and plant biochemistry at the University of Alberta, is the acting head; the division of industrial engineering, the division of textiles, the division of standards, and such other divisions as improvement in industrial processes, the development of natural resources, and the utilization of waste require.

Dr. Boyle was graduated from McGill University in 1906, and from then until 1909, when he received the Ph.D. degree and the 1851 scholarship, he did research

[1] From *Nature.*

on the properties of matter and radioactivity. From 1909 until 1911 he continued his work under the direction of Sir Ernest Rutherford at the University of Manchester. Returning to Canada, he lectured at McGill, was appointed assistant professor in 1912, and in the same year was made professor in the University of Alberta. During the war years, on the recommendation of Sir Ernest Rutherford, Dr. Boyle was engaged in research for the Admiralty Board of Invention and the Anti-submarine Division; and in that work he developed important applications of ultrasonics. In 1924 he tested apparatus for the detecting of icebergs and the sounding of depths in the Belle Isle Straits.

Dr. Whitby studied chemistry under Sir William Tilden at the Imperial College of Science and Technology, London, graduating in 1906 with the Frank Hatton prize. He was one of the first scientific workers to study the rubber industry, and one of his books thereon, "Plantation Rubber and the Testing of Rubber," 1920, has markedly influenced the trend of rubber research. In recognition of his contribution in that field, the Institution of the Rubber Industry (Great Britain) recently awarded him the Colwyn gold medal. In 1928 the distinction of Officier d'Académie was conferred upon him by the Government of France. The same year he was president of the Canadian Chemical Association.

As assistant director of the division of physics and engineering physics, Prof. John Hamilton Parkin, associate professor of mechanical engineering at the University of Toronto, has been appointed to direct the development of national aeronautical research laboratories.

Plans for the new National Laboratories building call for completion early in 1931. Meanwhile, temporary laboratory space has been provided.

THE AMERICAN STANDARDS ASSOCIATION

Announcement that the underwriting of the finances of the American Standards Association for a period of three years to permit a total annual expenditure of $150,000 for its work is now being completed has just been made by William J. Serrill, president of the association. This fund permits an increase in the budget for 1930 of $80,000 over the previous budget of the association and is expected to result in an expansion of national standardization work affecting practically all industries.

The fund is being underwritten by a large group of industrial organizations. The underwriting was arranged by a committee consisting of James A. Farrell, president of the United States Steel Corporation; Gerard Swope, president of the General Electric Company; George B. Cortelyou, president of the Consolidated Gas Company of New York, and F. A. Merrick, president of the Westinghouse Electric and Manufacturing Company.

Because of the rapid growth of the industrial standardization movement in this country, the underwriting was planned to permit immediate expansion of the work of providing authoritative national standards while permanent financing is under way. It is expected that this financing will be completed during the three-year period of the underwriting.

Among the companies joining in the underwriting are:

Aluminum Company of America
American Telephone and Telegraph Company
Bethlehem Steel Company
Consolidated Gas Company of New York
Detroit Edison Company
General Electric Company
General Motors Corporation
Gulf Oil Corporation of Pennsylvania
Public Service Corporation of New Jersey
Standard Oil Company of New Jersey
U. S. Steel Corporation
Westinghouse Electric and Manufacturing Company
Youngstown Sheet and Tube Company

Up to the present time the association has adopted approximately 160 national standards, and 190 other national standards are being formulated. The association provides the machinery by which all of the producing, distributing and consuming groups concerned with a standard may cooperate in its preparation. The foremost technicians of all groups are thus brought together to pool their knowledge for the benefit of all. Over 2,000 individuals representing 800 cooperating organizations are in this way working on technical committees under the procedure of the association.

An important feature of the association's work is the adoption of national standard safety codes, which are used voluntarily by industries and also as the basis for state and municipal safety regulations and for the regulations of insurance companies in numerous states. Among the most important of these codes are the National Electrical Safety Code, the Code for Mechanical Power Transmission and several codes for mine safety.

As the result of the recent affiliation of the American Home Economics Association with the American Standards Association, this latter body has also begun important standardization work on projects of direct concern to the ultimate consumer, such as refrigerators, sheets and blankets.

INTERNATIONAL CRITICAL TABLES

The sixth volume of the International Critical Tables was issued about the middle of October. The

permanent secretary of the National Research Council reports that most of the material for the seventh volume is now in type. It is not possible, however, to say when this volume will be ready for distribution for the reason that this, being the final volume of the series, will contain the general index, which will probably require several months for its preparation, since it is to appear in four languages, and much time must be allowed for translation and attendant correspondence in its compilation.

However, the Council is now in the final stage of this undertaking, the editorial work on which began in the summer of 1922, and the collection of funds two years earlier. Altogether about $205,000 has been contributed by industrial corporations and philanthropic organizations toward the editorial work of the tables. Royalties on the five volumes published prior to this fall amounting to $45,078.75 have been received and applied to the editorial expenses. With the receipt of royalties on the later volumes the remaining editorial work can be fully provided for. Together with interest received on funds temporarily invested and from other miscellaneous sources a total sum of about $235,000 will have been expended through a period of eight years on the editorial preparation of these tables. The total number of subscriptions to the tables at the prepublication rate of $7 per volume is about 6,700, and at the regular price of $12 per volume about 400. The total number of paid-up subscribers in both classes for the first five volumes of the series is about 6,300. Of these about 5,000 have subscribed for Volumes VI and VII.

The Council of the National Academy has approved the appointment of the committee recommended at the last meeting of the executive board to pass upon requests to reproduce material from the tables. This committee consists of the editor-in-chief of the tables, the permanent secretary of the council, and the chairmen of the council's divisions of physical sciences, and of chemistry and chemical technology.

RECENT APPOINTMENTS AT THE MELLON INSTITUTE

DR. E. R. WEIDLEIN, director of Mellon Institute of Industrial Research, Pittsburgh, Pennsylvania, has announced the following appointments at that institution made between July 1, 1929, and January 1, 1930. The appointments for the first half of 1929 appeared in the August 2 number of SCIENCE on page 115.

Senior industrial fellow:

James N. Lawrence, Ph.D. (Wisconsin, '12), has been appointed to the senior incumbency of the multiple industrial fellowship on wax. He succeeds Dr. D. K. Tressler, who has joined the research staff of the General Foods Company, Gloucester, Massachusetts.

Industrial fellows:

Henry A. Ambrose, Ph.D. (Mass. Inst. Tech., '30), has been appointed an industrial fellow on the multiple fellowship on petroleum production.

N. J. Beaber, Ph.D. (Iowa State College, '25), has been appointed to the nicotine fellowship. He was previously engaged at the institute in research on the gum fellowship.

Alexander C. Brown, M.S. (Mass. Inst. Tech., '26), has been added to the personnel of the multiple industrial fellowship on fatty acids. Before going to Pittsburgh, he had spent a year in Germany and two years in industrial work on petroleum.

Jasper S. Brown, B.S. (Maine, '26), has been appointed to the multiple fellowship on petroleum production. His previous experience was gained at the Pittsburgh Station of the U. S. Bureau of Mines.

Frank L. Jones, M.A. (Columbia University, '27), has been appointed to the enamels fellowship. Before accepting this position he was engaged in work toward the doctorate at Columbia, and he has also had teaching and industrial experience. He is succeeding B. A. Rice, who has joined the Pfaudler Company.

Ronald B. McKinnis, Ph.D. (Pittsburgh, '30), has been appointed to the can fellowship.

S. M. Martin, Jr., M.S. (North Carolina, '29), has joined the research staff of the multiple industrial fellowship on petroleum refining. Before going to the institute, he had had several years' experience in rubber technology.

Daniel C. L. Sherk, Ph.D. (Wisconsin, '20), has been appointed to the wood by-products fellowship. He has been engaged in industrial work since receiving his doctorate.

Thomas H. Swan, Ph.D. (Ohio State, '24), has been appointed to the garment fellowship. Since 1924 he has been the holder of the institute's bed fellowship.

A. J. Teplitz, B.S. (Kansas, '26), has been appointed an industrial fellow on the petroleum production fellowship.

Richard B. Unangst, B.S. (Lafayette, '16), has joined the personnel of the multiple utensil fellowship. Since 1916 he has been engaged in industrial work.

THE MICHIGAN ACADEMY OF SCIENCE, ARTS AND LETTERS

THE thirty-fifth annual meeting of the Michigan Academy of Science, Arts and Letters will be held in Ann Arbor on March 20, 21 and 22, 1930. The officers of the year are:

President—Oliver Kamm, Parke, Davis and Company, Detroit

Vice-president—Arthur E. R. Boak, University of Michigan

Secretary—Dow V. Baxter, University of Michigan

Treasurer—E. C. Prophet, University of Michigan

Editor—Peter Okkelberg, University of Michigan

Librarian—W. W. Bishop, University of Michigan

Section chairmen:

Anthropology, Fred Dustin, Saginaw

Botany, Frieda Cobb Blanchard, Ann Arbor

Economics and sociology, L. H. Seltzer, Detroit
Fine arts, Carleton Angell, Ann Arbor
Forestry, S. A. Graham, Ann Arbor
Geography, Wade DeVries, Lansing
Geology and mineralogy, Chester B. Slawson, Ann Arbor
History and political science, Howard B. Calderwood, Ann Arbor
Language and literature, F. W. Peterson, Ann Arbor
Mathematics, R. C. Shellenberger, Bay City
Sanitary and medical science, W. L. Mallmann, East Lansing
Psychology, W. C. Trow, Ann Arbor
Zoology, R. A. Muttkowski, Detroit

PRESENTATION OF THE JOHN FRITZ MEDAL

THE John Fritz medal was presented to Dr. Ralph Modjeski, member of the American Society of Civil Engineers, of New York and Chicago, at the annual banquet and reception of the society in the Hotel Commodore, New York City, on the evening of January 15. Approximately six hundred members and guests of the society were present. Immediately following the dinner Mr. Harrison P. Eddy, member of the society, as master of ceremonies, presented the newly elected president of the society and three honorary members, and then resigned the chair to Bancroft Gherardi, past-president of the American Institute of Electrical Engineers, as chairman of the John Fritz Medal Board of Award.

Mr. Gherardi spoke briefly of the purposes and history of the medal and introduced Mr. J. V. W. Reynders, past-chairman of the board of award and past-president of the American Institute of Mining and Metallurgical Engineers. Mr. Reynders summarized the achievements of the twenty-five preceding medalists, in subject groups, and then outlined Dr. Modjeski's personal history and emphasized his contribution to the art of bridge building, especially during the period of construction of great bridges in the United States.

Chairman Gherardi then presented Past-President Dexter S. Kimball, of the American Society of Mechanical Engineers, as chairman of the board which made the award to Dr. Modjeski, who spoke of the great contributions to human life made by engineers and scientists in the fields of machine tools, application of power, transportation and communication. He also alluded briefly to great contributions of the sanitary engineer and of the medical research men for the benefit of public health and of the engineering educator to the general advancement of the practice of the profession.

Following his address and in accordance with established custom, Dr. Kimball, as chairman of the board, presented the medal and certificate to Dr. Modjeski for "notable achievement as an engineer of great bridges combining the principles of strength and beauty." Dr. Modjeski responded briefly, accepting the honor.

SCIENTIFIC NOTES AND NEWS

As was reported in a recent issue of SCIENCE, Professor Michelson, having recovered from a serious attack of pneumonia, expects to return soon to Pasadena in order to continue by new methods his measurements of the velocity of light. The following appears in the London *Times* for January 8: "The death of Professor A. A. Michelson at the age of seventy-seven, announced at a recent meeting of the Académie des Sciences of the Institut de France, of which he was a corresponding member, deprives the scientific world of a physicist of original genius and remarkable achievement." There follows an extended obituary notice. Reference is made here to the matter in order that in so far as possible the anxiety caused by these announcements may be relieved.

A LESS serious error has occurred through the circumstance that it was cabled by the Associated Press from Stockholm that the Nobel Prize in physics had been awarded to the Duc de Broglie for his work on "Wave Mechanics." Duc Maurice de Broglie has accomplished distinguished work on X-rays, including their diffraction by crystals and the proof of Einstein's photoelectric equation in the region of X-ray frequencies, which in the opinion of physicists would warrant the award of a Nobel Prize to him. The work on the relation between waves and particles has, however, been accomplished by the younger brother of the Duc, Louis de Broglie, and it is to him that the Nobel Prize has been awarded.

DR. J. C. ARTHUR, professor emeritus of botany at Purdue University, celebrated his eightieth birthday on January 11. At a luncheon held in his honor by the staff of the department of botany of the Agricultural Experiment Station, Dr. Arthur gave an account of the earlier work and the development of the department of which he was head from its founding in 1888 to his retirement in 1915. Since retiring Dr. Arthur has been actively engaged in a continuation of his studies of rusts, having just published a book entitled "Plant Rusts."

MONDAY, January 13, marked the twenty-fifth anniversary of Dr. P. A. Levene's connection with the

Rockefeller Institute for Medical Research. The occasion was celebrated by a luncheon at the institute, attended by the entire staff and by many former coworkers of Dr. Levene, some of whom traveled great distances to be present. Brief addresses were made by Dr. Simon Flexner, director of the institute, and by Drs. W. A. Jacobs and Alexis Carrel, of the institute staff. A handsomely bound eight-volume set of his published articles was presented to Dr. Levene, who concluded the celebration with an address on "The Aims and Tools of Organic Chemistry and Biochemistry."

DR. FRIEDRICH KÜSTNER, director emeritus of the Bonn Observatory, has been made an honorary member of the American Astronomical Society, according to the provision that permits the election of one such member each year. Seven other living astronomers have been thus honored: Dyson, Charlier, Turner, Baillaud, Eddington, Wolf and Deslandres. A correspondent writes: "Küstner is best known for his detection, in 1888, of the variation of latitude, from observations made with a zenith telescope at Berlin. His two fundamental star catalogues, carried out with the meridian circle at Bonn, are unexcelled in accuracy and thoroughness, and are universally regarded as models for this class of work. With the comparatively modest equipment of the Bonn Observatory he has carried out a long series of determinations of radial velocities, and has shown that large telescopes are not necessary to secure results of a high order of accuracy. As a by-product of this work he was the first to determine the parallax of the sun by observing radial velocities of stars."

J. B. TYRRELL, consulting mining engineer and explorer, of Toronto, has been awarded the Daly medal of the American Geographical Society for his work in geography. The presentation was made by Dr. Isaiah Bowman, of New York, director and editor of the society, at a luncheon tendered Mr. Tyrrell by Sir Francis Younghusband, the British explorer. The David Livingstone Centenary medal was previously awarded to Commander Richard E. Byrd. This medal was not awarded by the National Geographic Society as previously stated.

THE Harrison Memorial lecture before the Pharmaceutical Society of Great Britain was delivered on January 14 by Mr. P. A. W. Self, who was presented by the president of the society with the Harrison medal. This lectureship and medal were established to perpetuate the memory of Colonel E. F. Harrison, director of chemical warfare during the war, and a distinguished member of the Pharmaceutical Society. The prize founded by the Chemical Society in 1922, in memory of Colonel Harrison, has been awarded to Dr. R. P. Linstead. This prize is awarded every three years to the chemist, under thirty years of age, who during the preceding five years has produced research work judged to be of the greatest merit and promise. The prize will be presented at the annual meeting of the Chemical Society in March.

AT the Des Moines meeting of the Mathematical Association of America, the Chauvenet prize of one hundred dollars was awarded to Professor T. H. Hildebrandt, of the University of Michigan, for his paper, "The Borel Theorem and its Generalizations" in the *Bulletin* of the American Mathematical Society for 1926. This prize is awarded every three or four years for the best expository paper published in English during the period of the preceding three or four years by a member of the association.

W. T. MACOUN, Dominion horticulturist, has been awarded the Wilder silver medal, given by the American Pomological Society annually, in recognition of his accomplishments in fruit breeding and horticulture generally.

R. HARCOURT, professor of chemistry at the Ontario Agricultural College, and A. T. Charron, assistant deputy minister of agriculture for Canada, have been honored by the French Government with the award of the Cross of Agricultural Merit. The award is the result of the visit to Canada in August, 1929, of a group of students and faculty from the National School of Agriculture at Grignon, France, and of the assistance given towards the success of that visit.

DR. A. S. EDDINGTON, Plumian professor of astronomy at the University of Cambridge, was elected president of the Mathematical Association, London, at the annual meeting on January 6.

MR. CARL E. GRUNSKY was elected president of the American Engineering Council at the meeting held in Washington on January 10.

DR. VERNON C. ROWLAND, associate professor of general and physical diagnosis, Western Reserve University Medical School, was recently elected president of the Academy of Medicine of Cleveland.

DR. ARTHUR H. LIBBY, Boston, was elected president of the Harvard Odontological Society for the coming year at the annual banquet of the association. Dr. Leroy M. S. Miner, of Boston, was chosen president-elect.

OFFICERS of the Philosophical Society of Washington have been elected as follows: *President,* W. D. Lambert, of the U. S. Coast and Geodetic Survey; *vice-presidents,* F. E. Wright and H. L. Curtis; *corresponding secretary,* L. V. Judson; *recording secretary,* O. S. Adams; *treasurer,* N. H. Heck.

In recognition of services rendered to the New York Academy of Medicine, Dr. Arnold C. Klebs, of Nyon, Switzerland, has been appointed consulting librarian of bibliography, and Professor B. W. Weinberger, of New York University, consulting librarian of dental literature.

Mr. Thomas A. Edison, Mr. Clarence Lewis, and Dr. E. D. Merrill, newly elected director, were elected members of the board at a recent meeting of the trustees of the New York Botanical Garden. The present officers, excepting the secretary, Dr. Nathaniel L. Britton, retiring director-in-chief, were reelected. Dr. Merrill was elected to succeed Dr. Britton as secretary and Dr. Marshall A. Howe was chosen assistant secretary.

Dr. Sidney W. Bliss has been appointed head of the department of biochemistry at Tulane University of Louisiana School of Medicine, New Orleans.

Fred C. Meier, extension pathologist, Office of Cooperative Extension Work, Extension Service, has been transferred to the position of principal pathologist in charge of barberry-eradication work of the Bureau of Plant Industry. This work has been separated from the Office of Cereal Crops and Diseases, Bureau of Plant Industry, and hereafter will be developed as a separate office of the bureau. Reorganization of the work will be undertaken immediately by Mr. Meier, with the idea of developing a more definite federal and regional basis of operation, although continuing close cooperation with the thirteen spring-wheat states and with the conference for the Prevention of Grain Rust. Dr. E. C. Stakman, of the department of plant pathology and botany of the University of Minnesota, who has long been identified with the black-stem-rust problem of wheat, especially with the epidemiological and related research, will act in an advisory capacity.

Dr. Harlan H. York, for seven years connected with the New York State Conservation Department, has resigned to accept the chair of forest pathology at the University of Pennsylvania. Dr. York was formerly a specialist in forestry at Brown University and the University of West Virginia and is chairman of the National Shade Tree Conference.

Oscar T. Quimby, chemist on the staff of the U. S. Forest Products Laboratory, assigned to experiments on the electro-endosmosis of wood, has resigned to take a position in the research division of the Procter and Gamble Company, Cincinnati.

Dr. Alexander Nelson, formerly superintendent of research in the Department of Agriculture in Tasmania, has been appointed lecturer in the department of botany of the University of Cambridge, and Dr. W. H. McCrea, formerly senior scholar of Trinity College and Isaac Newton fellow in the University of Cambridge, has been appointed lecturer in the department of mathematics.

W. H. Hoover, of the Smithsonian Institution, has returned from a three-year stay at Mount Brukkaros in South Africa, where he has been carrying on solar radiation investigations for the Smithsonian Institution and the National Geographic Society.

Dr. Rudolph Matas, emeritus professor of general and clinical surgery, Tulane University of Louisiana School of Medicine, has returned from a tour of Europe of several months' duration which was undertaken following his attendance at the International Congress of Surgery at Warsaw.

Dr. T. Shido, professor in the Kyushu Imperial University, and Dr. K. Iwakawa, professor in the Niigata Imperial Medical College, left in October to familiarize themselves with medical education in America and Europe.

Dr. Masayoshi Sato, professor of dairy chemistry, Hokkaido Imperial University, Japan, visited the United States in December on his return to Japan from a meeting of the executive committee of the International Dairy Federation held in Paris in October. The next World's Dairy Congress will be held in Copenhagen in June, 1931.

Dr. S. A. Waksman, soil microbiologist of the New Jersey Experiment Station, who has returned from Europe where for several months he has visited laboratories and experiment stations where microbiological work is being carried on, recently addressed the technical staff of the Bureau of Chemistry and Soils in Washington.

The council of the University of Melbourne has appointed Dr. R. Marshall Allan to the chair of obstetrics. Dr. Allan will devote the whole of his time to the organization of teaching and research.

Dr. M. Miura, a member of the Physical and Chemical Research Institute, Tokyo, died on November 20.

Dr. C.-E. A. Winslow, head of the department of public health at Yale Medical School and since 1927 assessor of the health committee of the League of Nations, delivered the second Aldred lecture at the Massachusetts Institute of Technology on January 17, when he discussed "Health Conservation—A Problem in Citizenship." Dr. Winslow also addressed the faculty club, speaking on "Ten Years' Work of the League of Nations."

Dr. Leon J. Cole, professor of genetics at the University of Wisconsin, will give a series of lectures open to graduate students in biology during

the summer session of 1930 at Western Reserve University. These lectures will deal with two related subjects, namely, "Genetics of Populations" and "Genetics and Evolution."

J. L. ST. JOHN, state chemist and head of the division of chemistry, Agricultural Experiment Station, State College of Washington, Pullman, is arranging a trip to be undertaken by Dr. H. E. Howe, editor of *Industrial and Engineering Chemistry*, for the purpose of addressing various groups at certain educational institutions, local sections of the American Chemical Society and other audiences. It is anticipated that the tour will begin the first of February, extend through the northern tier of states to Seattle, thence south to Los Angeles, and eastward through the southwestern states, occupying in all about six weeks. Those who may be interested in this project are invited to correspond with Professor St. John.

DR. M. M. LEIGHTON, chief of the Illinois State Geological Survey, Urbana, began a series of lectures at the department of geology, Northwestern University, on December 4 and 5. Dr. Leighton had for his subject "The Weathering Characteristics of the Glacial Drift Sheets of the Mississippi Valley States." On December 5, under the auspices of the Dip and Strike Club of Northwestern University, he delivered a lecture of popular nature on "The Economic Importance of the Glacial Drift of Illinois." The series was continued on December 10 and 11, with lectures on "The Relations of the Loess Deposits to the Various Drift Sheets and Their Chronology," and on "The Drainage of Ice Sheets and Its Influence on Morainal Building."

PROFESSOR DOUGLAS JOHNSON, of Columbia University, has recently delivered addresses in South Africa, Australia and New Zealand as follows: "The Face of the Waters," British Association, Cape Town; "Methods of Physiographic Research," geological department, University of Perth; "Studies in Shoreline Physiography," geological section of the Royal Society of New South Wales, Sydney; "Shore Problems," Royal Society of Queensland, Brisbane; "Physiography of the Atlantic Shoreline," geological section of the Philosophical Society, Wellington.

PROFESSOR P. EHRENFEST, of the University of Leiden, has accepted an invitation to participate in the Symposium on Theoretical Physics at the University of Michigan during the coming summer. In addition to giving a series of lectures, he will direct the informal conferences which accompany the lectures. Subsequently Professor Ehrenfest will visit several other universities throughout the country.

THE General Education Board, New York, has authorized grants to Duke University School of Medicine, amounting to $300,000 over a period of five years. It is also announced by the dean that the new medical school will open on October 1, and will admit first and third year students. The school will follow the four-quarter plan.

DISCUSSION

OVERHEAD SOUNDS OF THE YELLOWSTONE LAKE REGION

IN *Nature Notes from Yellowstone Park*,[1] Mr. L. S. Morris, ranger naturalist, describes aerial sounds which were heard by himself and companions over Grebe Lake, near the canyon of the Yellowstone in the park.

After reading Mr. Morris's narrative, I looked over a communication of my own to SCIENCE.[2] I also reread the notes upon which that communication was based.

Since there are some items in these notes which were not included in the published account it seems to me, especially in view of recently awakened interest in these phenomena, to be worth while to make a record of them.

Following are all the references to overhead sounds which I find in the diary which I kept during the six weeks of our stay in the park. I copy from the diary without making any changes in the text, such comments as seem to be called for being enclosed in parentheses.

(1) July 23 (1890). Yesterday, when (Elwood) Hofer and I were on our way to the upper (western) end of the lake (Shoshone), I heard a strange noise, which I supposed was off to the southward and echoing among the mountains. (At the time, about 8 A. M., I was seated in our Osgood canvas boat, with the oars in my hands, but not rowing. I was probably ten or twelve feet from the shore, where Hofer was seated measuring off our dredge rope, which we were going to use for a sounding line. As I remember the situation we were from twenty to thirty feet apart.) Hofer asked me what I thought it was, and where it seemed to be. I told him the apparent direction and asked him what it was. He replied that it was the most mysterious sound that was heard in the mountains. Since then we have talked about the sound a good deal in camp, and this morning heard it again very plainly. (My recollection of this event is that it occurred just after we had had breakfast, and

[1] 6: 2–4.
[2] First series 22: 244–6, November 3, 1893.

before we had separated for the day's work.) This time it appeared to be directly overhead, and to pass off across the sky, growing fainter and fainter toward the southwest. Hofer and Dave Rhodes, both of whom have had wide experience in the mountains, agree in their testimony in regard to it. They say they have never heard it anywhere out of the park, except to the south, about the forty-fourth parallel, some thirty miles south of here. He (Hofer) does not remember to have heard it farther west than Shoshone Lake, or east than Yellowstone Lake—not to the north of these points. It is heard mostly in the morning, shortly after sunrise, and up to, perhaps, half past eight, or nine o'clock. Hofer says he has heard it in the middle of the day, but usually not later than 10 o'clock A. M., doesn't remember to have heard it before sunrise. The description given of the sound before I heard it and since agree with my observations with regard to it. (The meaning of this somewhat obscure sentence is that the descriptions which Hofer and Rhodes give of the sound as they have heard it on previous occasions and their description of the sound as they have heard it here on Shoshone Lake agree with my own observations.) When heard best it appears to be a rather indefinite, reverberating sound in the sky, with a slight metallic resonance, which begins, or at least is at first perceived, overhead; at least, nearly every one in attempting to locate it turns his head to one side and glances upward. (I remember that while I was having my first experience with this sound in the sky I noticed that Hofer was watching me very closely. Later I found that he had been observing my reactions. He told me that people invariably behaved that way when they were trying to locate the source of the sound. I had at first looked up, and then had tried to follow the diminishing sound toward the southwest.) The sound is as difficult to describe as an echo, which has been repeated several times in quick succession. Each time I have heard (it) here on Lake Shoshone it appeared to begin to the southward, or, when first noticed, beginning overhead, or, as some one (Dave Rhodes) expressed it, "all over," and moving off toward the south.

(2) Camp on the "Thumb," Yellowstone Lake. August 4. While out on the lake this morning Professor Forbes and I heard again the strange noise which we heard several times on Shoshone Lake. It was about 8 A. M., morning still and clear, lake quiet, sun beginning to shine with considerable power (this mention of overhead sounds was preceded by an entry, which may, of course, have nothing to do with this phenomenon: "minimum temperature last night 33.5° F."); sound loudest almost overhead—seemed to pass to the southeast (so it stands in my diary. Since my other entries, where the apparent direction of these elusive sounds is recorded, indicate a direction west of south, which is in accord with my recollection of these events, I am inclined to think that this may be an example of those slips which Oliver Wendell Holmes cites in "Over the Teacups," where one writes north when he means south, and the like); sound of same nature as that heard on Shoshone (Lake)—very hard to describe—a certain metallic resonance—Professor Forbes calls it a kind of twisting, yow-yow vibration, resemblance to sound made by telegraph wires, but not a steady, uniform volume. The sound lasted probably half a minute, time not noted. As I have heard the sound here it seemed to begin at a distance, something like a mixture of wind in pine tops, in telegraph wires, the echo of bells, after being repeated several times, the humming of a swarm of bees and two or three other sources of sound, all making a not loud, but easily recognized sound, not at all likely to be mistaken for any other sound, but easily overlooked if one is surrounded by noises. The party on shore heard the same sound at the same time that we heard it on the lake. (On this occasion Professor Forbes and I were in our canvas boat, one hundred yards, more or less, from shore. Hofer was on the beach at the water's edge. The others were at the camp, which was some fifty feet or more back from the edge of the lake terrace, in a grove of pine-trees. I have a kodak picture of the scene which was taken from our boat as we were nearing shore on our return from this trip.) Hofer says he doesn't remember to have heard it when the sky was cloudy, has heard it when "quite considerable breeze" was blowing. Remembers that he has usually heard it when the sky is clear, or with few clouds, and the morning calm; as a rule in the morning, but has heard it as late as noon. Dave Rhodes thinks he has heard it only in the mornings, and when the sky is clear, or with light, fleecy clouds.

(3) August 8. Professor Forbes and I rowed up to Bridge Bay (northwest end of Yellowstone Lake), where we collected—back (to hotel) about 12:30 (P. M.).... We heard our mysterious sound again this morning, at 10 and 10:15, while out (on the lake) collecting. There is reason to believe that it is caused by the steamboat *Geyser* (on east shore of the lake), and heard through some peculiar condition of the atmosphere at distances of several, perhaps thirty to fifty miles away. (Here I seem to have arrived at that state of mind where consolation is derived from belief in a theory.)

(4) August 9. 2:20 P. M., at head of southeast arm of (Yellowstone) lake. While in boat heard sound overhead, like rushing wind traveling very rapidly or like something rushing through the air, did not have the semimetallic sound, or like echo; seemed to travel from east to west; clear, except light, fleecy and feathery clouds, enough wind to ruffle the surface of the water. (On this occasion I was one of a party of five which had left the Lake Hotel on the afternoon of the eighth in two rowboats, with tent and camp outfit. My companions, who were engaged on the construction of a new building, were Mr. L. D. Boothe and Messrs. Couglin, Curl and Thomson. Mr. Curl and I were in one boat, the three others in the other. We were making our way from the east to the west side of the east arm of the lake, and were rowing slowly in the very shallow water of this part of the lake when the sound attracted our attention.)

The following note is included in Mr. Morris's article: "In the *Ranger Naturalists' Manual* for 1928 there appears a rather complete summary of the re-

corded observations of this weird phenomenon by Ranger Marguerite Arnold."

EDWIN LINTON

ZOOLOGICAL LABORATORY,
UNIVERSITY OF PENNSYLVANIA

THE NORMALITY OF THE MATURATION DIVISIONS IN THE MALE OF DROSOPHILA MELANOGASTER

IN a recent article in SCIENCE, "Recent Discussions of the Reduction Division in *Drosophila melanogaster*," Jeffrey[1] has assailed Belar's[2] conclusion that the maturation phenomena in this form are normal. For the past six months the writer has been studying maturation and allied phenomena in the male gonads of *D. melanogaster* and, since my observations seem to explain quite simply the anomalies reported by Jeffrey in his preparations, a brief description of my results is given below.

The gonads of larvae of varying ages were quickly dissected in a 2 per cent. urea solution and transferred immediately to strong Fleming's fluid containing 1 per cent. of urea by volume. After fifteen to thirty minutes the tissue was placed in Herman's fluid containing 1 per cent. urea and fixed for three hours. This is the technique used by Painter in his translocation studies.

When primary spermatocytes stained with iron hematoxylin were first studied, the number of darkly staining elements observed was larger than was expected from the diploid chromosome number in the male, but further examination revealed a great variation in the number and size of these stained bodies. On close study, these structures could be separated into two groups, the first made up of the four tetrads, identified by their shape, and the second containing the other bodies which did not stain so intensely as the tetrads and whose shape was usually spherical. Often these spherical globules appeared quite hollow, and they exhibited no definite relation to the equatorial plane. It was variation in the amount of this second type of material which gave the impression of variable chromosome number. As these observations suggested that the material included in the second group was not chromatin, differential strains were used. After being stained with Auerbach's acid fuchsin-methyl green, the tetrads were green, while the other elements were bright red. More extensive study of numerous preparations gave the following facts: (*a*) a very large acidophilic nucleolus is present in the growth period of the first spermatocyte. It is usually spherical but it may appear as a mass of globules. (*b*) There is no regularity in the time at which the nucleolus breaks up and loses its capacity to retain the stain. In some instances it ceases to stain before the first maturation spindle is formed, while in other cells it breaks up into a number of globules which take the stain well, even as late as the telophase of this division. This behavior of the plasmosome explains the variation in the amount of the apparent chromatin in the first maturation spindle. If it disintegrates before the spindle is formed, only the tetrads are present at the time of division, while if it has fragmented but has not lost its capacity for staining, the products lie in the region of the spindle and stain with iron hematoxylin, giving the appearance of true chromatin.

There are four tetrads in the first maturation spindle, conforming in size to what might be expected from the diploid chromosomes. They divide normally with the X and Y elements segregating to the opposite poles. If plasmosomes are present in the cell, they tend to be roughly distributed to the two poles, but they are never included in the new nucleus. By the second maturation division the plasmosomes have usually disappeared and the chromosomes are easily studied. Their division at this time is normal.

These facts seem to give a simple explanation of the figures published (and demonstrated) by Jeffrey. In his cells there were two types of material, chromosomes and plasmosomes; but, due perhaps to the preservative used, the true tetrads could not be identified by their shape, as in my material. The structures described by him as "chromosomes . . . far removed from the equatorial line" are obviously the same as the deep-staining bodies which I have found in similar cells and which give an acidophilic reaction with differential stains. The irregular distribution of the plasmosome material to the cytoplasm of the two daughter spermatocytes, in my opinion, has been misinterpreted by Jeffrey as the elimination of true chromatin.

From my observations I am forced to conclude that the maturation process in *Drosophila melanogaster* is normal as far as chromosomes are concerned. Why the plasmosomic material should show such variation in the time it loses its staining capacity is not clear, unless it be due to the great rapidity with which maturation is carried on in this form.

BESSIE B. LEAGUE

DEPARTMENT OF ZOOLOGY,
UNIVERSITY OF TEXAS

[1] SCIENCE, 70: 579–580, December 13, 1929.

[2] "Die cytologischen Grundlagen der Vererbung," Berlin, Gebrüder Borntraeger, 1928.

SAND-STORM ELECTRICITY

I HAVE read with much interest the discussions on atmospheric electricity in SCIENCE for March 30, 1928; May 3, 1929, and October 18, 1929. On June 23, 1927, I read a paper on "Some Remarkable Elec-

trical Conditions Accompanying West Texas Sand-storms" before the American Physical Society at Reno, Nevada, which paper was abstracted in the *Physical Review,* 30: 362, September, 1927.

Of the four papers mentioned above, it seems to me that only two refer to anything like similar conditions. The experiments of Brasch, Lange and Urban, in Switzerland, were made during thunder-storms. The air was probably quite free from dust. Nothing is said on that particular point, but one would expect the air to be quite pure in the mountains of Switzerland. The observations of Benade, in India, seem to have been made when a variety of atmospheric conditions were present such as sand, dust, rain and lightning. I take it for granted that the climate in that part of India is quite moist, although the author says nothing on that subject. The observations of Canfield in New Mexico and of George, Young and Hill at this school were made under cloudless skies when rain, thunder and lightning were conspicuous by their absence. As a matter of fact, west Texas sand-storms are generally accompanied by a humidity so low as to be almost unmeasurable. I believe it is generally understood that whatever the cause of sand-storm electricity may be, it is altogether different from that which is responsible for the electrical display during thunder-storms.

By reference to the above-mentioned abstract it will be observed that we obtained voltages about twice as high and currents about six times as great as those observed by Benade. As to the magnitude of the current, however, I think it probable that, under given conditions, it varies directly with the total area of the collector. That, however, remains to be verified. Our collector was a radio aerial consisting of a single stranded wire 33.75 meters long arranged east and west in a nearly horizontal position, with a lead-in wire about one third as long.

I think Benade is quite right in intimating that the effect is not due entirely to friction. Recently Dr. W. H. Abbitt, of this laboratory, has made observations which seem to indicate that an effect may be obtained during a sand-storm from a loop aerial located in a practically dust-proof laboratory. These observations, however, need confirmation.

The climatic conditions at Jornado Range are probably quite similar to those in west Texas, and Canfield's observations are very interesting to us here. I am inclined to think, however, that he is mistaken on one point, and that is with respect to the arc which he says he obtained. To maintain an arc three and one half centimeters long in the open air would require a current of quite a different order of magnitude from what I believe to be possible under the conditions of his experiment. Besides, the ends of the wire, under the intense heat of the arc, would have rapidly melted away. The melting-point of copper is not over 1080° Centigrade, while the temperature of the arc may run up to about 4000° Centigrade. I am of the opinion that what he observed was a succession of spark discharges which followed each other so rapidly that, due to persistence of vision, it seemed to be continuous. The noise, however, would probably be quite different from that of the arc. The glow which he obtained at night seems to have been a corona discharge.

There is a world of literature on the subject of atmospheric electricity dating back to and beyond the experiments of Lord Kelvin. An excellent résumé of the literature on the electrification of various kinds of dust and smoke is given in "Clouds and Smokes" by W. E. Gibbs published by J. and A. Churchill, London. But in spite of all that has been done in this field, a field pregnant with great potentialities, we have scarcely made a beginning. Dr. Abbitt has taken over the work on stand-storm electricity at this school. He is now working on a new type of apparatus for measuring the electrical potential of the atmosphere.

E. F. GEORGE

TEXAS TECHNOLOGICAL COLLEGE

ABSENT-MINDEDNESS AS A FACTOR IN PROFESSIONAL ETHICS

SEVERAL exceedingly interesting instances of professional ethics which have more or less recently come to my attention furnish food for thought. They are related here with essentially correct details but purposely disguised so that the authors, specialities and countries can not be identified; the purpose of this disguise (involving "not less than" in connection with the number of pages and number of illustrations) is to bring out underlying principles without embarrassing the authors. Upon relating these cases to some colleagues "in the club," a responsive chord was struck and I was urged to publish the instances.

The citations themselves can well be prefaced by the premise that practically all authors occasionally, however absent-mindedly and inadvertently, commit sins of omission or of commission. The instances here related represent somewhat extreme examples of absent-mindedness (if that be the correct technical term).

Case 1. An article of not less than forty pages, with not less than fifteen illustrations; ten of these are immediately recognizable to the initiated as copied from other authors, but no acknowledgment is given.

Case 2. An article of not less than six pages, with not less than eight illustrations; four of these are

copied from other authors; acknowledgment is made for two of them.

Case 3. An article of not less than thirty pages, with not less than ten illustrations; four of these are copied from other authors; acknowledgment of source is given for one illustration.

Case 4. An article of not less than twenty pages, with not less than ten figures; at least five of these are copied (one from a deceased author); source is stated for one, not stated for three; one of these figures is copyrighted by another publisher.

Case 5. A specimen was sent by a collector to a certain specialist for determination. The determination was made and a detailed drawing of it was prepared, involving certainly many hours of intensive work. The collector asked to borrow the drawing and the specialist was delighted to lend it to him. In a few weeks this drawing was published by the collector in a copyrighted journal with no reference as to its source.

Case 6. The prize case for "absent-mindedness" is an article of not less than fifty pages, with not less than twenty-five illustrations, all given as "original." Some of these figures bear a remarkable resemblance to old friends, but there is something unnatural about them. The artist explained this interesting puzzle. He said (in effect): When Dr. X— wants an illustration, and finds one to suit him, I photograph it, then draw the negative [reversed] view; for instance, a left view now shows as a right view, and *vice versa;* of course the illustrations thus become original drawings.

Many other cases might be cited, but the foregoing are sufficient to remind us all that possibly none of us is entirely free from absent-mindedness. A story is making the rounds of Washington that a man of non-scientific training heard of "professional ethics" and expressed surprise when he learned that this was not some sort of a skin disease. Is it possible he had in mind the condition known as pachydermia?

C. W. STILES

U. S. PUBLIC HEALTH SERVICE

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A MODIFIED FORM OF NON-ABSORBING VALVE FOR POROUS-CUP ATMOMETERS

As was first pointed out by Livingston,[1] the porous-porcelain atmometer must be provided with an arrangement to prevent water absorption through the porous evaporating surface in periods of rain, fog or dew formation. This necessitates the presence of a valve in the tube that connects the porous cup with the water reservoir below, the valve being so constructed as to allow movement of water upward while it practically prevents downward flow. Livingston's original mercury valve[1, 2] has been modified in various ways, and several forms of it are now in use.[3] Another form of valve is that of Livingston and Thone,[4] which is now generally used.

In all these valves excepting the one last mentioned, the downward hydrostatic pressure of the water column in the supply tube is balanced by a mercury column of equivalent hydrostatic pressure acting in the opposite direction when free water is in contact with the outside of the porous cup above, as in times of rain. Although differing in detail these all consist essentially of a U-tube containing mercury inserted in the water-supply line from reservoir to cup. When water is moving upward it forces the mercury into one arm of the U-tube and then passes around it. When absorption through the atmometer wall begins mercury rises in the other arm until the mercury column in that arm balances the downward pressure of the water, when absorption and backward flow are halted. With each closure of this type of valve a small amount of water enters the reservoir. If a number of valve reversals occur in a period of operation (as when periods of absorption and evaporation follow each other on a day of frequent showers) the error thus introduced may be considerable,[5] but its magnitude is so small as to be practically negligible in most instances. The absorption error for each reversal of a value of this type is, of course, a volume of water equal to the volume of mercury held in the second arm of the tube when the valve is closed, and the bore of the tube used for this arm should therefore be as small as is practicable.

The Livingston-Thone valve consists of two porous plugs a centimeter or two apart in the vertical supply tube, with a small mass of mercury enclosed between them and resting on the lower plug. As water moves upward it passes around the mercury, the column of which is only a few millimeters high, but downward flow is prevented because the mercury mass acts like an ordinary poppet valve, seating itself on the lower

[1] B. E. Livingston, "A Rain-correcting Atmometer for Ecological Instrumentation," *Plant World*, 13: 79–82, 1910.

[2] B. E. Livingston, "Atmometry and the Porous Cup Atmometer," *Plant World*, 18: 21–30, 51–74, 95–111, 143–149, 1915.

[3] Frank Thone, "Rainproofing Valves for Atmometers: A Résumé," *Ecology*, 5: 408–414, 1924.

[4] B. E. Livingston and Frank Thone, "A Simplified Non-absorbing Mounting for Porous Porcelain Atmometers," SCIENCE, 52: 85–87, 1920.

[5] E. M. Harvey, "The Action of the Rain-correcting Atmometer," *Plant World*, 16: 89–93, 1913.

plug, into the pores of which mercury does not enter to any considerable extent with the low hydrostatic pressure involved. The plugs are now commonly made of sheep's-wool yarn, and the absorption error of a valve of this type, when well made, is much smaller than the corresponding error in a valve of the U-tube type. However, this type of valve requires some attention from time to time, especially because the pores in the plugs are likely to become closed by algal growth or bacterial slime, which may accumulate rapidly at summer temperatures. When the upper plug becomes partially sealed the operation of the instrument may be prevented by the accumulation of gas between the two plugs. When the lower plug is too loose mercury may escape downward.

In the extensive evaporation survey now being carried on in Ohio, atmometers equipped with valves of the plug type required frequent inspection by an expert, and the valve described below has been used throughout the past season. It is of the U-tube type and is much like the valve used in the Shive mounting,[6] but is constructed to pass through the opening of an ordinary thirty-two-ounce bottle with small mouth.

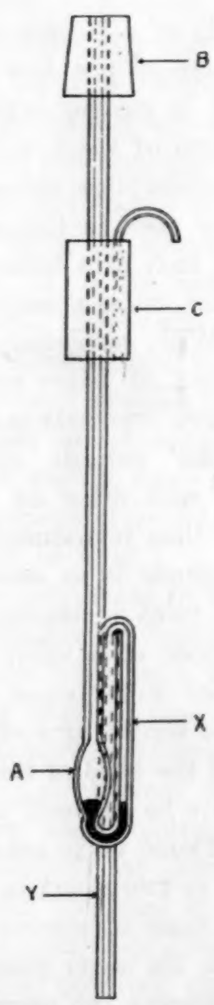

FIG. 1. Non-absorbing atmometer valve.

The accompanying diagram shows an atmometer mounting equipped with the new device. The rubber stopper *B* carries the atmometer sphere, and the cylindrical cork stopper *C* fits tightly in the bottle mouth. The cork carries a bent copper tube by which air is allowed to enter the bottle without danger of water entrance in rainy periods. The valve itself is continuous with the supply tube, the whole being made of glass barometer tubing having an outside diameter of 6 mm and a bore of about 1.5 mm. A piece of tubing about 41 cm long is used for each mounting. The valve consists essentially of a narrow loop made by two sharp bends, each of 180°, with a bulb *A* at the lower end of the long portion that leads directly to the atmometer sphere above. The first ascending part *X* is about 7 cm long, and the descending part *Y* is long enough so that the whole mounting is about 30 or 31 cm long. The bulb *A* has an internal diameter of at least 5 mm and a total length of about 2 or 2.5 cm. The capacity of the bulb is about 1 cc. The *X* and *Y* portions must be very close to the bulb, in order that the valve may pass through the 18-mm mouth of the bottle reservoir. The bend below the bulb should also be as close to the latter as possible so that the absorption error inherent in valves of this type may be small. The error in this valve is about 0.1 cc.

When evaporation is going on and water is ascending through the valve, the mercury (indicated as black in the diagram) all lies in the bulb *A* but does not fill it, and water flows upward around the mercury. As soon as absorption exceeds evaporation some of the mercury in the bulb passes around into the portion *X*, which has a small bore, until the hydrostatic pressure that produces absorption is balanced by the narrow mercury column extending above the level of the mercury meniscus in the bulb. It is seen that the parts *A* and *X* constitute the essential U-tube mentioned above.

To install this mounting the bulb is first filled about one third full of mercury. Then a piece of flexible rubber tubing, about 10 cm long and supplied with an open pinch cock, is attached to the lower end of the *Y* portion. The free end of this piece of tubing is then placed in water and gentle suction is applied at the upper end of the mounting until all gas in the system has been displaced by water. The pinch cock is then closed, the mounting inverted and the rubber stopper *B* tightly seated in the neck of the atmometer sphere, the latter being completely full of water. Finally, the mounting is returned to the upright position (with the sphere above), the rubber tube is removed and the mounting is quickly set into the reservoir bottle, which has been previously filled or nearly filled with water. After a few hours the reservoir may be filled to the mark on its neck for the beginning of a period of observation.

[6] J. W. Shive, "An Improved Non-absorbing Porous Cup Atmometer," *Plant World*, 18: 7–10, 1915.

About a hundred spherical atmometers with mountings like the one here described were in operation, in the Ohio survey mentioned above, during a nineteen-week period in 1929. With the exception of two, the valves of which were accidentally broken, all the instruments operated perfectly throughout the period without the attention of persons specially trained in caring for them.

J. D. WILSON

OHIO AGRICULTURAL EXPERIMENT STATION,
WOOSTER

THE USE OF N-BUTYL ALCOHOL IN DEHYDRATING WOODY TISSUE FOR PARAFFIN EMBEDDING

THE common procedure in dehydrating, clearing and embedding tissue for cytological examination is, first, to replace the water normally present in the specimen with ethyl alcohol. The alcohol is then replaced by some volatile fluid soluble both in it and in paraffin. In the preparation of plant material for sectioning this fluid is nearly always xylene, which is in turn replaced by melted paraffin. On solidification the paraffin holds and supports the tissue so that it can be sectioned properly.

There are certain limitations to the above technique which make it unsuited for animal cells and for any plant material which contains lignified elements. The higher concentrations of alcohol harden any specimen left in them too long, often before it is completely dehydrated. All the water must be extracted from the specimen before the xylene will penetrate, and this involves the use of absolute alcohol. The xylene itself causes animal cells to shrink and become brittle and so hardens the wood elements that they can not be cut but break and chip the edge of the microtome knife. These disadvantages have been overcome in the preparation of small zoological specimens by using clove oil, cedar oil, chloroform, etc., in place of xylene. Painter[1] has developed a method of substituting aniline oil for the higher concentrations of alcohol, replacing the aniline oil with methyl salicylate (oil of wintergreen) and passing from the latter into paraffin. This method does not harden wood provided the specimen is carried through very gradual changes. The several liquids diffuse so slowly, however, into half-inch cubes composed of xylem, cambium and phloem that the method is impractical for this material.

Mlle. Larbaud[2] has dehydrated and cleared with a mixture of ethyl and n-butyl alcohols. Butyl alcohol is soluble in paraffin in all proportions, but only 8.3 grams are soluble in 100 cc of water. A mixture of equal parts of ethyl and butyl alcohol, however, is completely miscible with water. Larbaud based her technique upon the ethyl alcohol-xylene series: 30 per cent., 60 per cent., 80 per cent., 95 per cent., absolute alcohol, 2 pts. absolute alcohol-1 pt. xylene, 1 pt. absolute alcohol-2 pts. xylene, xylene. She shortened the above eight stages to six by using equal parts of ethyl and butyl for the alcohol of the first four stages followed by two changes of pure butyl alcohol.

Small cubes of wood with cambium and phloem attached require more gradual dehydration. The following series of mixtures of water, ethyl and butyl alcohol has been satisfactory:

Water	95–89–82–70–50–30
Ethyl alcohol	5–11–18–30–40–50
Butyl alcohol	0– 0– 0– 0–10–20

One hour is generally enough for each step except the last. The material should remain over night in this solution which contains a total of 70 per cent. alcohol. It should be emphasized here that alcohol not only dehydrates the tissue but also, as a powerful reducing agent, completes chrome fixation by reducing the chromate ion ($^-CrO_4$) to the chromic (Cr^{+++}). Keeping the specimen in alcohol over night eliminates certain irregularities in the fixation images of various chromic compounds (Zirkle[3]).

The dehydration is completed by the stages:

Water	15– 5– 0– 0– 0
Ethyl alcohol	50–40–25– 0– 0
Butyl alcohol	35–55–75–100–100

An hour in each stage is generally sufficient except that the tissue should remain in the pure butyl alcohol until *all* the water is extracted.

As butyl alcohol dissolves solid paraffin extremely slowly, nothing is gained by placing chips of paraffin in the vial of alcohol that contains the specimen and by waiting for them to dissolve in the cold. A simple method is to fill a vial two thirds full of paraffin, let the paraffin harden and place the material to be embedded upon it. Cover the specimen with butyl alcohol and place the vial in the oven. As the paraffin melts, the tissue sinks. Butyl alcohol, being lighter than melted paraffin, does not sink with the specimen which consequently comes into intimate contact with almost pure paraffin. Two changes of paraffin are generally sufficient, the length of each change depending upon the size of the specimen.

In spite of the fact that butyl alcohol diffuses into paraffin more slowly than does xylene, it has several advantages over the latter as a clearing agent. Its specific gravity is less, being .810 at 20° C. compared with .881 for orthoxylene and .866 for metaxylene.

[1] *Anat. Record*, 27: 77–86, 1924.
[2] *Compt. Rend. Acad. Sci. Paris*, 172: 1317–1319, 1921.
[3] *Protoplasma*, 4: 201–227, 1928; 5: 511–534, 1929.

It is slightly lighter than paraffin at the latter's melting-point and when the specimen sinks, as described earlier, it remains floating on the paraffin. Xylene, on the other hand, is heavier than melted paraffin and sinks with and surrounds the specimen. There is an additional advantage in the use of butyl alcohol as slight traces of it in the paraffin blocks do not render them crumbly as does a like amount of xylene.

Butyl alcohol does not harden wood, and its use makes the higher concentrations of ethyl alcohol unnecessary. Any wood that can be sectioned green may be sectioned in paraffin if dehydrated, cleared and embedded as described above. Thus it is possible to get thin, smooth sections of the soft cambium and phloem even if hard xylem elements occur in the slices. Woods as hard as hickory (*Carya ovata*) and locust (*Robinia Pseudo-Acacia*) have been sectioned satisfactorily. The fine cytological details which can be investigated by this technique are destroyed by the prolonged hydrofluoric acid treatment, a necessary concomitant of the colloidin technique. However, butyl alcohol does not soften wood which has once been hardened by fixation, by drying, or by a too rapid dehydration, and all such material should be prepared for sectioning in other ways.

The simplicity of the butyl alcohol method, as pointed out by Larbaud, makes it preferable for other plant organs: buds, leaves, root-tips, etc. It is much quicker than the ethyl alcohol-xylene technique.

CONWAY ZIRKLE
ARNOLD ARBORETUM AND BUSSEY INSTITUTION
HARVARD UNIVERSITY

SPECIAL ARTICLES

GEOLOGICAL EVENTS IN THE HISTORY OF THE INDIO HILLS AND THE SALTON BASIN, SOUTHERN CALIFORNIA

THE landward extension of the long depression occupied by the Gulf of California has experienced a very eventful history during Tertiary time. In the course of an investigation of the Indio Hills, now in progress, several facts of geologic significance have been discovered.

The trough, extending northwestward for nearly three hundred miles from the head of the gulf, was formerly known as the Colorado Desert. Its topographic subdivisions, in order from the gulf northwestward to its head at San Gorgonio Pass which leads into the coastal portion of Southern California, are the Colorado River delta or alluvial cone, Imperial Valley, Salton Sink or Salton Sea, and Coachella Valley. It is flanked on the west throughout much of its length by high mountains, rising to a maximum height of about 10,000 feet; it sinks to about 275 feet below sea-level beneath the recently formed fresh-water Salton Sea. Tertiary marine and continental sedimentary formations outcrop at various places along the margins and bottom of the trough and yield clues to its origin and geological history.

The area of these deposits which we are studying, known as the Indio Hills, lies north of Indio, in central Riverside County. A similar neighboring area to the southeast, known as the Mecca Hills, is being investigated by Mr. Hampton Smith.

The Indio and Mecca Hills trend northwest-southeast, and lie end to end. Each of the two areas measures about twenty miles long by two to six miles wide, and together they extend most of the distance from the Salton Sea to the northwest end of the Coachella Valley. They occupy a position parallel to and only slightly northeast of the median line of the broad depression. The hills rise about 1,000 feet above the plain and in the very arid climate exhibit a magnificent badland topography.

Our investigation is part of a broad program of Tertiary history studies initiated by Dr. John C. Merriam and sponsored by the Carnegie Institution of Washington.

1. We find that the upper part of the Indio Hills block is constituted of two formations. One of these is marine and is doubtless the correlative of the Carrizo formation, named by W. S. W. Kew[1] with type locality on Carrizo Creek, southwest of Salton Sea. It outcrops in the Indio Hills as small areas at several localities both east and west of the mouth of Thousand Palms Canyon and in the northern part of the hills. It consists of yellow clays with some sandstone and conglomerate. Its age was determined as upper Miocene by Ralph Arnold[2] and as lower Pliocene or younger by T. Wayland Vaughan.[3] The formation, containing a fauna related to that living in the Gulf of California and very distinct from the west coast invertebrate assemblages of California, records an invasion of the Gulf of California over the Indio Hills block. This incursion has been determined by other workers to have extended almost to San Gorgonio Pass.

The second formation is several thousand feet thick and consists entirely of arid-climate, terrestrial de-

[1] "Tertiary Echinoids of the Carrizo Creek Region in the Colorado Desert," Univ. Calif. Publs., *Bull. Dept. Geol.*, 8: 2, 1914.
[2] U. S. Geol. Surv. Bull. 396, p. 44, 1909.
[3] U. S. Geol. Surv. Prof. Pap. 98, p. 369, 1917.

posits. Clays, probably playa deposits, arkosic sandstones and fanglomerates with considerably worn fragments form in sub-equal thicknesses nearly the entire exposed section in the hills. For this areally and stratigraphically important set of rocks the name Indio formation is proposed, its type section being taken along a northeast-southwest line through the Indio Hills about two miles northwest of Thousand Palm Canyon.

The Indio formation is part of the comprehensive Mud Hill Series of Free,[4] which includes all the Cenozoic strata of the region, both marine and continental. The marine portion was, as indicated, separated in 1914 as the Carrizo formation by Kew.

2. At all localities the Indio formation overlies the Carrizo formation. Considering both the striking difference in the nature of the sediments and in their mode of deposition the Indio is probably unconformable on the Carrizo.

3. We have secured no fossil material in the Indio formation, but its stratigraphic relations to the Carrizo formation on the one hand and the well-indurated character of the Indio formation on the other indicate a probable age not greater than middle Miocene and not less than lower Pliocene; in short, approximately middle Neocene.

4. The San Andreas fault, heretofore thought to skirt the northeast margin of the hills, has recently been located quite independently by Dr. Levi F. Noble in the course of his elaborate study of that important structural feature, and by us. It crosses the northwest end of the hills obliquely and then follows the southwest base.

5. Structurally the hills are strongly folded, and our mapping of axes indicates that the folds are not parallel to the margins or trend of the hills but strike somewhat more westerly so that they are cut off obliquely on the west by the bordering San Andreas fault. An interesting but as yet somewhat speculative inference from the trend of the folds is that while the initial and earlier movements on the San Andreas fault in this region may have been due to forces acting roughly parallel to it, the folding and more recent movement on the fault—northwestward shifting of the southwestern block relative to the northeastern—have probably been caused by forces acting from the south and at a considerable angle to the fault. It is quite possible that the eastward swing of the San Andreas fault in the Indio Hills region is related to the shortening across the Indio Hills block indicated by the folding. The above explanation for the folding of course relates the folding and the San Andreas faulting as to date; this is plausible, for while the folding occurred some considerable time ago, as indicated by the deep erosion of the hills, the San Andreas fault, though now active, is also known to have suffered movement intermittently far back in Tertiary time.

[4] Carnegie Institution of Washington, Publication 193, p. 23, 1914.

6. In a number of references, including a recent text-book, the Salton Basin is asserted to have been occupied so recently by the Gulf of California that it was separated from it by the building of the Colorado delta or alluvial cone. It appears to us much more probable, however, that the depression below sea-level of the present Salton Basin has occurred during or since the building of the delta or cone, not before, and long after the last invasion by the sea.

The possibility of concurrent depression and delta building, as contrasted with pre-delta depression, was first pointed out and discussed by Free[5] but not urged, probably because at that time less information was available regarding the age and structural relations of the formations.

The belief that the depression of the basin and the delta or cone building were contemporaneous and that the delta did not cut off an arm of the sea is based upon the following considerations.

(*a*) The delta or cone of the Colorado River is relatively young geologically, probably Quaternary in age, since it was superposed as a feature upon a landscape which already resembled the existing one, although not identical with it. The lowest line across the cone along which the sea might enter the Salton Basin, along the intersection of the west slope of the cone and the west wall of the basin, rises to only about thirty feet above sea-level. Hence, under the old view, the basin would have been connected with the gulf until the last or upper thirty feet of the cone was added. This fact coupled with the youthfulness of the cone would make the latest presence of marine or brackish waters an event of Recent or Quaternary time. But while deposits of a relatively recent fresh-water lake fed by the Colorado are excellently exposed in the basin, no marine beds of sufficiently late age or bearing expectable relations to the topography and to the sea-level contour have been anywhere encountered by the writers in journeys about the basin nor are such reported in the literature.

(*b*) While terraces referable to the recent fresh-water lake are conspicuous, no marine terrace or strand has anywhere been noted by the writers nor have such features been recorded by others.

(*c*) The marine strata in the basin—the Carrizo formation of Miocene or lower Pliocene age—are much older than the Colorado cone.

(*d*) The position of the marine strata hundreds of feet above sea-level and the fact that they are strongly

[5] Carnegie Institution of Washington, Publication 193, pp. 25–29, 1914.

folded and beveled by erosion indicate that they were not deposited during an occupation of the present basin by the sea but in an earlier depression of considerably different form, and long before the present basin was formed.

(*e*) The strongly folded and erosionally beveled Indio formation records a long interval of continental deposition, deformation and degradation between the deposition of the underlying marine Carrizo and the development of the landscape which truncates the Indio and on which the Colorado cone—heretofore regarded as the dam which cut off the sea—was built.

(*f*) The very late date for the depression of the Salton Basin below sea-level is made the more probable by abundant evidence of recent movement in the form of very young fault scarps cutting alluvial fans and occasional earthquakes.

It might at first sight appear somewhat improbable that, in a sinking trough, the Colorado should maintain a cone now only about thirty feet above sea-level at its lowest point and yet high enough continuously to exclude the gulf out of the area sinking below sea-level to the northwest. But only two alternatives were possible if the floor of the trough sank from above to below sea-level: either the river was or was not able to upbuild its cone as rapidly as the floor subsided tectonically and to maintain it as a dam. The size of the stream, its unusually heavy burden of sediment and the 125-mile extension of the cone which it has built southward to the head of the gulf give ample ground for believing that the river would be able to cope with the sinking of the trough and maintain the dam. If any part of the cone sank, through tectonic movements, somewhat below the normal grade line of the river, which was necessarily always above sea-level, that portion would soon be built up again during one of the frequent swings of the river down the different radii of its cone.

That the salt in the bottom of Salton Basin was in all probability derived from the evaporation of Colorado River water and not from a cut-off arm of the sea has been shown by W. H. Ross.[6]

In the light of our observations and present recorded knowledge we consider it highly probable that, instead of being a cut-off and desiccated arm of the Gulf of California, the present Salton Basin sank below sea-level while the Colorado River excluded the gulf by maintaining a dam in the form of a huge alluvial cone across the southern portion of the basin.

JOHN P. BUWALDA
W. LAYTON STANTON

BALCH GRADUATE SCHOOL OF
THE GEOLOGICAL SCIENCES,
CALIFORNIA INSTITUTE OF TECHNOLOGY

[6] Carnegie Institution of Washington, Publication 193, pp. 45–46, 1914.

SEX GLANDS AND ADAPTIVE ABILITY[1]

THE purpose of our investigation[2] was to determine the relation between sex glands and adaptive ability. As the first of a series of experimental reports, the present paper deals with the effect of castration upon problem box and maze performances by the white rat. The term castration is used here in its narrow sense, meaning extirpation of the testicles of male animals. It does not include ovariectomy of the female, which will form the subject of a separate experiment.

As soon as time permits, we shall attack the same general problem by studying the effects of heat, X-ray, vitamin E deficiency, testicular elevation and the ligation of vas deferens. Some of these experiments are now in progress. Besides these, we plan to study the effect of injection of testicular extracts as well as the effect of feeding interstitial tissue. These two problems will be studied along with implantation and transplantation with reference to such questions as organotherapy and rejuvenation.

The maze and the problem box were used in our experiment because they offer two widely different problematic situations to our animals and at the same time represent the best recognized technique we have in comparative psychology. White rats were chosen as our subjects. In the future, we hope to repeat our experiments on higher animals such as dogs, monkeys, etc.

Twenty-seven male rats were used in our original experiments. They were of approximately the same age and body weight. All of them had learned Maze A in an earlier experiment. Operation was performed at the age of about five months. Twelve rats had both of their testicles removed (total castration), eight had only one testicle removed (semi-castration), while the remaining seven were put through a sham operation without being castrated, that is, an operative control. Preliminary feeding was given once daily for one week, after the operation, at the center of a single-platform problem box. Six of the totally castrated rats and the seven control animals were tested in the morning, while the other six totally castrated and the eight semi-castrated ones were left for the afternoon. Each animal was confronted with the problem box situation once daily for four weeks. Bread and milk were used as the incentive. Time was the only criterion that could be employed. It was taken with a stop-watch. The results for the problem box experiment are presented in Table I.

These data on the problem box experiment justify the following statements.

[1] Read before the Ninth International Congress of Psychology held at New Haven, Connecticut, September, 1929.

[2] The author expresses his sincere gratitude to Professor Harvey A. Carr for kind aid and interest in this research.

TABLE I

AVERAGE NUMBER OF SECONDS REQUIRED FOR SOLVING THE PROBLEM BOX

Daily average by weeks	Morning groups		Afternoon groups	
	Total castration 6 rats	Operative control 7 rats	Total castration 6 rats	Semi-castration 8 rats
First week	497	90	668	236
Second week	59	33	112	32
Third week	32	12	68	37
Fourth week	23	7.4	19	17
4 weeks Av.	153	35.6	217	80.5

(1) For the morning groups, the totally castrated rats show a poorer record than the control animals in their adaptive ability as measured by the time required to solve the problem box. The difference is very large and consistent throughout the four weeks under investigation.

(2) For the afternoon groups, the totally castrated rats show a poorer record than the semi-castrated animals in their speed of solving the problem box. The difference persists throughout the four weeks studied.

(3) The totally castrated rats of the afternoon groups show a poorer record than the totally castrated rats of the morning groups during the first three weeks, after which the difference is reversed. For a considerable time previous to the experiment, the afternoon groups were accustomed to be fed with the other groups in the morning. When the experiment began, however, it was found necessary to work on them in the afternoon. This change in their feeding habit may have been responsible for the poorer initial record of the afternoon group.

TABLE II

ORIGINAL SCORES FOR MAZE PERFORMANCE

	Mornings groups		Afternoon groups	
	Total castration 6 rats	Operative control 7 rats	Total castration 6 rats	Semi-castration 8 rats
Av. of first week—				
No. of errors	32	15.8	24	20.4
No. of retracings	18.5	6.8	11	8.7
Time in seconds	1,679.8	558.8	922.5	790.1
Av. of second week—				
No. of errors	7.3	3.3	8.1	6.4
No. of retracings	1.3	.3	.5	.4
Time in seconds	213	143	217	174
Total—				
No. of errors	39.3	19.1	32.1	26.8
No. of retracings	19.8	7.1	11.5	9.1
Time in seconds	1,892.8	701.8	1,139.5	964.1

One day after the problem box experiment, the animals were fed at the food box of Maze B for one week and trained in the maze once daily during the next two weeks. Number of cul-de-sac errors, number of retracings and time per trial were recorded. The results for maze performance are presented in Table II.

The experiment on maze performance was repeated on sixteen more animals. Eight of them were totally castrated while the other eight served as control. None of them had any previous experience with problem box or maze performance. These further results are presented in Table III.

TABLE III

FURTHER SCORES FOR MAZE PERFORMANCE

Average score for 4 weeks	Total castration 8 rats	Operative control 8 rats
No. of errors	64.4	41.7
No. of retracings	30	13.6
Time in seconds	1,873.4	1,252.2

The above data on maze performance justify the following statements.

(1) The totally castrated rats show a poorer record than the control animals in both speed and accuracy of maze performance. This difference persists throughout the two weeks tested for the morning groups of the original experiment and is also borne out by the average score for four weeks of the repeated experiment.

(2) The totally castrated rats show a poorer record than the semi-castrated animals in both speed and accuracy of maze performance. The difference persists throughout the two weeks studied.

To conclude, our data show that the totally castrated rats are inferior to the semi-castrated and the control animals in their adaptive ability as measured by the speed in solving a single-platform problem box as well as by both speed and accuracy in maze performance. As to whether the difference is due to the direct effect of the sex hormone upon the nervous system or to the change in metabolism as measured by body weight, respiratory quotient, nitrogen in the urine, etc., we shall leave for further investigation. However, Professor I. P. Pavlov's experiments, as reported at the Ninth International Congress of Psychology, on the effect of the sex hormone upon the excitability of nerve centers seem to support very strongly our first

theory, namely, the direct effect of the sex hormone upon the nervous system.

LOH SENG TSAI

THE OTHO S. A. SPRAGUE MEMORIAL INSTITUTE, UNIVERSITY OF CHICAGO

TRANSMISSION AND DIFFRACTION OF LIGHT BY NORMAL SERUM AS A FUNCTION OF THE TEMPERATURE

THE present paper is a résumé of the results obtained by a further step in the systematical study of the psychochemical changes undergone by normal blood serum as a function of temperature.

We have shown previously[1] that: first, an absolute minimum of the viscosity was observed around 56° C., and that the specific viscosity increased rapidly after 58°; second, that the levo-rotatory power, unaffected by temperature up to 55° (for ten minutes' heating) increased abruptly, and that the increase, up to a certain point, was proportional to the temperature. Measurement of the amounts of light absorbed, and scattered at right angle by the molecules and particles in the serum, brought new evidence of the deep physicochemical changes which take place around 55°. The table expresses the results of one series of experiments. (Figures express the readings.)

Heated for	0	5	10	20	40	60 min.	
Temperature			52°				
Transmission	57,5	57,5	57,5	57,5	57,5	57,5	
Diffusion	125	125	125	125	125	125	
Temp.			55°				
Transmission	57,5	57,5	57,5	57,5	57,5	57,5	
Diffusion	125	125	125	125	125	125	
Temp.			57°				
Transmission	57,5	57,5	58,0	58,0	59,0	60,0	
Diffusion	125	125	117	108	95	88	
Temp.			58°				
Transmission	57,5	57,5	58,0	58,5	61,0	61,0	
Diffusion	125	122	115	105	83	83	
Temp.			60°				
Transmission	57,5	57,5	58,0	60,0	63,0	70,0	
Diffusion	125	105	96	85	75	56	
Temp.			62°				
Transmission	57,5	59,0	60,0	65,0	90,0	101,0	
Diffusion	125	95	83	65	53	46	(coag)
Temp.			64°				
Transmission	57,5	60,0	63,0	96,0	140,0		
Diffusion	125	90	73	56	48	(coag)	

In both cases (transmission and diffusion) the figures are proportional to $\log \frac{I_0}{I}$, I_0 being the incident light, and I the transmitted or the scattered light. In the case of scattered light, the ratio $\frac{I_0}{I}$ for a reading of 125 is approximately equal to $\frac{1.000.000}{1}$. This table shows plainly that, if the optical density of the solution, $\log \frac{I_0}{I}$, increases slowly after 55° is reached, the same ratio decreases very rapidly when it applies to the diffracted light. (Of course, *decreasing* figures express *increases* in the amount of scattered light.)

Ten minutes' heating at 55° determines no increase in the scattered light, but ten minutes at 60° increases it roughly threefold. By applying Lord Rayleigh's formula connecting the amount of scattered light to the volume of the scattering particle, it is possible to express the increase in scattered light in terms of increased volume of the particle.

When this is done, and the data for different temperatures plotted against the time of heating, curves are obtained which show a decided tendency towards flattening when forty minutes are reached. On the contrary, if the same data are plotted against temperature, the curves are parallel and the increase in volume of particles, when the serum is heated for five, ten, twenty and forty minutes, is very nearly proportional to the temperature; *i.e.*, an increase in temperature of one degree determines about the same increase in the volume of the particle, between 57° and 64° C., whether the heating has lasted ten, twenty, forty or sixty minutes. The proportionality is rigorous within the limits of experimental errors—for a heating of ten or twenty minutes. A discrepancy is observed when the heating is kept for forty and sixty minutes. These curves are parallel to those expressing the changes in rotatory power.

One of the practical conclusions to be drawn from these findings is that the study of diffracted light affords a much more sensitive method for the study of phenomena occurring in the serum than that of transmitted light.

P. LECOMTE DU NOÜY

INSTITUT PASTEUR, PARIS

[1] P. L. du Noüy, *J. Gen. Physiol.*, 1929, xii, 363; Ann. Inst. Pasteur, 1928, xlii, 742 and 1929, xliii, 749; SCIENCE, 1929, lxix, 552.

BOOKS RECEIVED

CALVERT, ROBERT. *Diatomaceous Earth.* Pp. 251. 70 figs. 46 tables. Chemical Catalog. $5.00.

CHAMOT, ÉMILE MONNIN, and CLYDE WALTER MASON. *Handbook of Chemical Microscopy*—Volume I. Principles and Uses of Microscopes and Accessories—Physical Methods for the Study of Chemical Problems. Pp. xiii + 474. 162 figs. Wiley. $4.50.

MILLER, CASPER O. *The Ether in its Relation to the Structure of Matter.* Pp. 240. 35 figs. Henkel Press. $3.50.

RIES, H. *Economic Geology.* Sixth edition, revised. Pp. 860. 291 figs. Wiley. $6.00.